THE
PHILOSOPHICAL MAGAZINE
AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY,

AGRICULTURE,

MANUFACTURES, AND COMMERCE.

BY ALEXANDER TILLOCH, LL.D.

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Monit. Polit.* lib. i. cap. 1.

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THE PHILOSOPHICAL MAGAZINE AND JOURNAL.

I. *On the Flower-buds of Trees passing through the Wood, as noticed by Cicero and Pliny. By Mrs.* Agnes Ibbetson.

To Dr. Tilloch.

Sir, — Some late dissections of wood have enabled me to notice the curious manner in which the flower-buds pass *layer* by *layer* through the wood even to the root, and have shown me that each mark is peculiar to the sort of wood to which it belongs. Thus, in the Oak, the bud being sessile, or without stalk, and in large numbers together; they generally appear grouped in a circle, as at Plate I. fig. 1; and it is hardly possible to pass through the wood, and then take fibre from fibre, without encountering innumerable buds thus passing up from the root perpendicularly, or crossing the stem at right angles to its former direction. As it is in old wood torn down, not cut, the gastric juice (which always precedes the bud) is rarely seen, though its effects are most visible and remain permanently so; for, if a set of buds have to cross a knot, many holes are perceived in the knot through which they have passed, and in which the gastric juice has formed them a passage; but which do not close again as the wood usually does, because of the hardness of the parts around the knot. In the Beech, where the buds follow each other in a sort of *laxus racemi*, it presents a very different picture. Here the buds being small, they will run up between the layers of the wood, and are not so conspicuous as in the Oak; though when the wood is *torn up, not cut*, the whole number show with peculiar grace, as forming a sort of stripe of apparent flowers, which the figure of the bud produces, thus passing up perpendicularly (fig. 2.). In the Yew, they are an assemblage which shows buds of all ages, many just peeping through the wood, others more ad-

vanced towards the bark; but all generally surrounding an old one (fig. 3.): an innumerable assemblage that are hastening on to the bark. What should cause some buds to proceed all the way up the wood perpendicularly, and others to cross at once to the bark, I cannot conceive, and have never been able to guess: but so it is. The Olive shows like one large peaked bud, appearing at some little distance from each other; but I suspect that it is a collection, since it carries that divided appearance when it is followed into the interior. It is certain the wood-lines diverge (fig. 4, *aa*) in a manner that proves that innumerable buds are hourly passing, for the yearly lines never move out of the circle, but to effect this purpose:—a most striking circumstance.

That any person can *deny a fact so evident* to sight "as the passing of the flower-bud through the wood, even from the root upwards, is most strange. But when I add, that not one botanist in a thousand has really examined the woods when newly uncovered by the bark, and then followed it with the knife as far as its marks go through the ligneous part; I advance only what my experience teaches, and what they will not I fancy deny: for many, when they saw the specimens I showed, were astonished, allowed the evidence to be complete, and the facts to be just as I had represented them. Thus, it is only those that do not see my specimens, who do not believe in the system: nor am I surprised, when I recollect with what indifference we view all novel objects, though ever so beautiful, till some *interest* draws our attention to them; that the mind rarely accompanies the eyes in the investigation, till our curiosity is *excited*, and our thoughts turned into that channel;—then the whole breaks at once upon us, *truth* becomes *conspicuous*, and we are astonished we did not remark it sooner. How few are there whose mind always accompanies their eyes, who cannot perambulate Nature's garden without noticing each plant, or each *unpractised* figure, which presents itself, not before seen! How few are there whose mind will always be alive to every novel object; who, when they discover it, *must follow it* in a progressive manner through every part of the picture, till they have made themselves masters of the subject, and not allow prejudice to stand between them and truth! To walk with such a man through Nature's garden, is indeed a treat, yet but rarely met with; not a tree, not a leaf, but presents something curious to the eye of the observer, and brings its observation with it. It is scarcely to be believed how carelessly my work has been noticed, and how little botanists are agreed on the subject, except in the wish to get rid of it, when it is certainly a *new science* which they have not yet *examined*, but which might prove (if well followed up) of the greatest utility to both farming

ing* and gardening, besides displaying so exact an analogy between the animal and vegetable world.

In the various comments made on my work, many said it was absolutely impossible the bud should come from the root, that there was no occasion to dissect wood to be convinced of that proposition; in short, it was affirmed in as many words that there was no occasion to examine woods; that they possessed so perfect and so intuitive a knowledge of nature independent of inquiry, as made it *unnecessary*. Another gentleman assured my friend that the fact was *well known*, and exactly described by Sir J. Smith. To put an end to this objection, I shall transcribe his own observation in his work on physiological botany. He says, "Mr. Knight in the Philosophical Transactions for 1805 has shown that buds originate in the *alburnum* next the bark, as might indeed have been expected." This seems to show that Sir J. Smith is of the same opinion. This opinion is certainly very different from mine, as I have repeatedly shown that they protrude from the root. Willdenow thought that they were formed in the bark. Du Hamel gave no decided opinion on the subject. How these gentlemen could suppose they passed through the bark first, when the round head of the bud first appears peeping through the wood, I cannot conceive. Grew alone has announced that he has seen the bud pass up through the middle of the plant in the interior, full six months before it shows itself at the exterior of the plant: he must therefore have seen it in the root; for the new shoot, at the top of which they afterwards appear, could not at that time be formed. How then should the bud be protruded there? 'Tis plain, therefore, it appears, even at the exterior, first in a lower part of the plant.

The buds passing from the root will alone explain some curious passages to be found in Cicero and Pliny, where they describe the situation of the tree when the buds were running up: and it is plain that the secret of cutting down the tree at the *proper season*, was carefully preserved by the few who possessed it, with the most strict attention paid to the *time*; otherwise the price of the wood could never have been raised to the enormous height it was. Small tables made from the root of the trees so

* The Flemish farmers find their weeds not only drawn for them, but taken off, and the ground thoroughly weeded by *hand labour in spring*; and the weeds, instead of being turned back into the ground, are collected and boiled for the milch cows, when green food is so scarce and difficult to be had, without expense. The farmers thus get their land weeded for nothing by the neighbouring poor, for the purpose of procuring the food for their cattle; and those very poor who have not cattle, are paid for gathering it for those that have. The farmer is thus freed from a nuisance, and the food is excellent. This might be admirably done in Devon and Somersetshire, where such nourishing weeds (according to the soil) are constantly found.

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marked or spotted, were the mania of that period, and even the grave Cicero yielded to the folly.

Pliny's description of the lesser Maple (the ancient *Bruscum*) is well worth citing in the original; and immediately brought to my mind the different figures of the roots of various trees when cut down at the proper season; for this does not last above a *fortnight* or *three weeks* at most, in any *tree;* but if taken within that time, most roots form a very beautiful picture which explains many passages in both authors. The trees which have this property are the Yew, the Citron, and the Maple, not only the Italian but the French one. Pliny's description is: "Acer, operum elegantiâ et subtilitate Citro secundum, Gallicum in Transpadana Italia transque Alpes nascens. Alterum genus, crispo macularum discursu, qui, cùm excellentior fuit, a similitudine caudæ pavonum nomen accepit." There are several kinds, especially the white, which is wonderfully beautiful. This is called the French Maple, growing in that part of Italy that is on the other side of the Po, beyond the Alps. The other has a curled appearance so curious (fig. 5), that from a near resemblance it was usually called the Peacock's Tail. Lib. xvi. c. 16. It is very curious that I should have some of this curled figured wood so exactly described by Pliny, in many foreign woods (fig. 7) as well as the Maples, and in the Bird's Eye American Maple, which of course they could not then know, that directly showed me what Pliny meant. He goes on to comment on those of Istria, and those trees growing on the mountains, and esteemed the best, and to sing forth the praises of the Bruscum knots. But the Molluscum was counted by the Romans as the most precious. I have among my Indian woods many specimens admirably marked; I have also one which rises up in stripes, and bears the appearance of a Fir within (fig. 6). They are not large enough to make any thing but the ladies' sets of tables in fashion a few years past, but served to stand by the couches of the Romans when they dined, or after dinner; which gives a higher idea of their luxurious customs than any fact I have yet read of them. But I hope I shall never live to see that extravagance imitated in this country, which gave rise to the curious Roman saying common among the gentlemen of Rome, when they exclaimed that the ladies had "turned the tables on them." As when they reproached the ladies with the expense of their jewels and ornaments, the ladies reminded them of the tables that had often cost from six to ten thousand sesterces; even the grave Cicero gave, I think, and boasts of giving, eight thousand for a set, and they must have been small.

The Bruscum is more intricately crisped and curled than the Molluscum (fig. 8); but the planks are larger and the pattern is fuller; "and had we," says Cicero, "trees to make

or saw into broader planks, they would be preferred to Citron." I have some very beautiful specimens of the Ash that takes a perfect polish; it had the exact resemblance of a large crab, or rather spider, which was not only displayed in the root, but showed itself in one of the branches of a smaller size. I had also one of Beech that was in regular stripes of green, brown, or pale yellow, constantly flowing from the iron and copper which must have been nearly under the tap root, and which plainly proves in what an exact line the sap flows: although this is not the general opinion, it certainly evinces that there is no aperture to let the sap pass from one layer of wood to the other; but that each is completely inclosed within its own cylinder. If it was not so, indeed, how could poison flow in one cylinder, and a perfectly insipid liquid in the next, as in the Laudanum plant? or a strong caustic in one layer, as in the Ranunculus, and a totally innocent juice in the adjoining layer? I could name a hundred plants in which the same circumstances occur. "The knots and interior parts of the timber of the trees which produce at this season the bruscum," says Cicero, "most resemble the female Cypress;" except, he might have added, that the buds cross the wood as well as run up it perpendicularly, which is not the case in the abovementioned tree. The bruscum is of a blackish wood with larger buds. "I have a piece," says Cicero, "from which the molluscum came, which is most perfect," so that he called any trees thus that were so marked. The famous *Tigrine* and *Pantherine* curiosities, are tables spotted or made of the roots of trees while the buds were passing up; but the curious circumstance of numbers hunting for some pieces, and finding them quite plain though taken from the same sort of tree in which another who knew the secret had at an earlier season succeeded, formed a sort of marvellous discovery that caused the price to be kept up (I suppose) *in Rome*. I should never have found out the time myself, but from so often cutting the buds on the outward bark, or rind, to discover the season at which the nucleus of the bud entered under the scales in the bark. When the nucleus could not be found, and nevertheless the scales appeared on the bark, I was sure it must be the time to cut down the tree: and when I cut open the root they were all within it ready to run up, and pass under their scales. By delaying therefore one fortnight the tree being cut down, I soon found both the molluscum and bruscum of Pliny: and taking a fresh tree of the same kind a month after, and cutting open the buds, the nucleus was within them, and a very few remained scattered in the root and up the bark; they had therefore repaired to the scales at the exterior. I found in the root of the Lime tree, which affects a very rich loam, a most beautifully arranged

ranged figure, with a little stretch of the imagination we might suppose a peacock, or at least the tail: the spots are larger than in the Maple, and the curl more decided. In some Indian woods the small bud is almost hidden in the flourishes round it. The *Tilia* is, I believe, supposed to be the *Philyra* of the ancients. They used to make bottles of it; and it is mentioned that they were often seen spotted, and that the spots often fell out. This is so exact a description of the bark and alburnum, with which they were made, and of the hearts of the seeds falling out, that much is gained to my present studies by examining with care into every thing that has vegetables for its object, when either Pliny, Virgil, or Cicero mention that subject. I think I have heard of the paper of the *Tilia* being as good as that of the *Betula alba*, and I have repeatedly tried; but the hearts of the seeds are so strongly impressed upon it, that I could never make it bear the writing on both sides, which I have effected with the *Betula alba* when well prepared and pressed. Yet I have found a passage in one of my books of observations, of a work of Cicero, "*De ordinanda republica,*" written on this species of paper formed from the *Tilia*, and now in the public library at Vienna. I possess a root which I suppose to be one of the greatest I have. It is the root of an Elm of the small leaves, one which never flowers in this country though common in our hedges: it is hollow, though with a thick exterior; in the middle an immense bud projects six inches in circumference, and the root is nearly twelve inches in diameter. The bud when cut perpendicularly down, shows a quantity of the nucleus of the flower or bud, only not covered with the scales of the bark. It was sent me by a gentleman not conversant in botany, but quick of observation, who found it in one of the lanes adjoining Exmouth. Fig. 10.

II. *Observations on Naphthaline, a peculiar Substance resembling a concrete essential Oil, which is apparently produced during the Decomposition of Coal Tar by Exposure to a red Heat. By* J. Kidd, *M.D. Professor of Chemistry, Oxford. Communicated by* W. H. Wollaston, *M.D. F.R.S.**

Although the existence, and many of the properties of the substance above mentioned, have been already noticed in two of the Philosophical Journals of this country †, there has not yet appeared, as far as I can discover, any systematic description of

* From the Transactions of the Royal Society for 1821, Part II.

† Thomson's Annals of Philosophy, January 1820, page 74; and Mr. Brande's Quarterly Journal, January 1820, page 287.

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the mode by which it may be obtained, or of its relation to the substance from which it is produced; on which account I have been induced to offer to the Royal Society the following observations respecting these points of its history.

In the experiments which led, in the present instance, to the detection of the substance in question, it was proposed to effect the decomposition of coal tar, by passing its vapour through an ignited iron tube; and, in order to increase to the utmost the extent of the ignited surface, that portion of the tube which was constantly kept up to a red heat, was filled, in the first instance, with a series of hollow iron cylinders open at both extremities, and successively decreasing in diameter, so as to be included one within another. In other instances these cylinders were removed, and their place supplied by sand, or by pieces of well burnt coke, or by pieces of brick; but it was found that the interstices between the cylinders, or between the particles of sand, &c. were so soon choked up with carbon from the decomposition of the tar, as to be rendered absolutely impervious to the gas produced during the decomposition; so that it became necessary to pass the vapour of the tar simply through the tube itself.

Connected with the tube in which the tar was decomposed was a vessel, in which any undecomposed vapour of the tar, or any products resulting from its decomposition, might be condensed; and at the end of every experiment this condensing vessel was found to contain an aqueous fluid having an ammoniacal odour, and a dark coloured liquid resembling tar in appearance.

This dark coloured liquid is characterized by the following properties:

Its colour, in the mass, is black; but when spread in a thin stratum on paper or glass, it is of a clear deep reddish brown colour.

It is a much thinner liquid than the coal tar from which it was produced; and has a peculiar and slightly aromatic odour, together with the smell of ammonia; about three-fourths of a given quantity of it pass through unsized paper; and that which remains on the paper resembles common tar. Sp. gr. 1050; the sp. gr. of the tar from which it was produced being 1109.

Readily and entirely soluble in ether.

Soluble, but not entirely, in alcohol; the solution becoming milky upon the addition of water, and this milky mixture passing unaltered through the pores of the closest filtering paper.

Not miscible with water; but readily communicating to it a light brown colour, and a taste at first sweet, but followed by an aromatic pungency. The water acquires alkaline properties, and holds

holds ammonia in solution. When poured out on a flat surface, it catches fire almost immediately on the application of flame, and burns for a time exactly in the same manner as a thin stratum of alcohol, the flame being blue and lambent, and without smoke; but after a few seconds the flame becomes white, and the liquid begins to burn with much black smoke, and with a crackling noise.

A pint of this dark coloured liquid was submitted to very slow distillation in a lage glass restort connected with a large glass receiver, from the interior of which all communication with the external air was excluded by means of a common safety valve. The heat was supplied from the flame of an Argand gas burner, and was so slight as scarcely to inconvenience the naked hand, when held over it immediately under the bottom of the retort.

The same degree of heat was applied constantly during forty hours; at the end of which time there had distilled into the receiver rather more than half a pint of a liquid, which consisted of two perfectly distinct portions, which, however, had uniformly passed over together from the very commencement of the distillation.

The uppermost of these portions, in appearance, resembled pale olive oil, and amounted to not quite a quarter of a pint. The lowermost portion resembled water, but was not perfectly transparent, and amounted to rather more than a quarter of a pint: but there is ground for believing, from the results of subsequent distillations, that the proportion of the aqueous product is variable; and that it is greater when the distillation is carried on slowly, than when it is carried on rapidly.

After the above-mentioned products had passed over, a concrete substance as white as snow began to collect in dispersed crystalline flocculi, in the upper part of the body and neck of the retort, so as in a short time almost wholly to obstruct the passage; the oily fluid and the water continuing to pass over at the same time, but much more slowly than before.

At the end of sixty hours the original quantity of the dark coloured liquid was reduced to about a quarter of a pint; and what remained was much thickened in consistence: the heat was therefore increased: and now there began to pass over a darker coloured and thicker oil, which, as it advanced further from the source of heat, congealed into a substance of the consistence of butter. The heat being still more increased, this oil became darker coloured and more dense; and when at the last there remained in the retort not above one-eighth of the quantity originally poured into it, and the heat of the gas burner had been increased to the utmost, there arose a heavy yellow vapour, which was condensed in the neck of the retort in the form of a farina of a bright yellow colour.

When it appeared that the heat no longer separated any thing from the black matter in the retort, which still however retained a degree of fluidity, the apparatus was suffered to cool; during which time the residuum became fixed, and to the eye resembled pitch.

The several products of the distillation above described being carefully separated from each other, the more remarkable of them were submitted to examination; but as leisure was wanting for a full investigation of their characters, the Society is requested to accept, with some indulgence, the following description of such of their properties as were ascertained.

Properties of the aqueous Product.

Taste, saline and alkaline; with an ammoniacal and slightly aromatic odour.

Sp. gr. 1023.

Became faintly blue by the addition of a solution of prussiate of potash.

Grs. 700 of this aqueous fluid were evaporated under an exhausted receiver inclosing a quantity of dry muriate of lime: the residuum of the 700 grains weighed not more than half a grain, and consisted partly of a brown oil and partly of a sparingly soluble saline matter, which by the proper tests was found to contain sulphuric acid and muriatic acid; the former apparently in greater quantity than the latter.

Properties of the oily Fluid.

Taste, pungent, bituminous, and aromatic; with an odour similar to the taste, and slightly ammoniacal.

Sp. gr. 0·9204.

Boils at about 210° of Fahrenheit: remains perfectly fluid at 32°.

Evaporated at a medium atmospheric temperature, it leaves about one-sixth of its weight of the peculiar concrete substance, which will be described in the next section: by the assistance of heat, dissolves about one-third its own weight of that substance.

Readily catches fire upon the application of flame, and emits a very great quantity of smoke while burning.

By agitation mixes temporarily with water at the common temperature; from which however it soon separates like oil.

Slightly soluble in boiling water; but in cooling is deposited so as to give a milky appearance to the water, which remains perfectly transparent while at or near the boiling point.

Unites readily with alcohol and with ether at all temperatures.

By agitation with an aqueous solution of potash, or of ammonia, it communicates a slight wheyishness to those fluids; but soon separates from and floats on the top of them.

Absorbs several times its volume of ammoniacal gas, without any sensible change.

Absorbs also several times its volume of muriatic acid gas; becoming, in consequence, opaque and thick.

Forms a uniform white soapy curd with a solution of acetate of lead, by the invervention of an aqueous solution of potash or of ammonia; but, if simply mixed with the metallic solution, it soon separates without any sensible change.

Properties of the white concrete Substance.

Taste, pungent and aromatic.

It is particularly characterized by its odour, which is faintly aromatic, and not unlike that of the narcissus and some other fragrant flowers. This odour is readily diffused through the surrounding atmosphere to the distance of several feet, and obstinately adheres for a long time to any substance to which it has been communicated.

When in its purest state, and reduced to powder, it is exceedingly smooth and slightly unctuous to the touch; is perfectly white, and of a silvery lustre.

Sp. gr. rather greater than that of water.

It does not very readily evaporate at the common atmospherical temperature: for, a comparison being made between this substance and camphor, in the quantity of half a grain of each in a very minute state of division, it was found that the camphor had entirely disappeared at the end of 18 hours, while the substance in question had not disappeared entirely at the end of four days.

A quantity of it being exposed to heat, in a glass vessel, soon melted; but did not begin to boil till the temperature had reached 410° of Fahrenheit: the heat being then withdrawn, it remained liquid till cooled down to 180; at which point the lowest portion was seen suddenly to congeal; the remaining portion congealed gradually; and when the whole had become solid, its temperature was 170°. The structure of the congealed mass was distinctly crystalline, and the crystalline laminæ were slightly flexible.

It is not very readily inflamed; but when inflamed it burns rapidly, and emits an unusually copious and dense smoke, which soon breaks into distinct particles that fall down in every direction.

Does not affect the colour either of litmus or of turmeric.

Insoluble in cold water; and very sparingly soluble in boiling water,

water, from which it separates, in cooling, in such a manner as to render the water milky, which was before transparent: a portion however still remains dissolved, for the water, when filtered, possesses in a slight degree the taste and odour of the substance, and after a few hours deposits it in minute crystals.

Readily soluble in alcohol, and still more so in ether, at any temperature; the solubility, in either instance, greatly increased by increase of temperature.

A solution of this substance in four times its weight of boiling alcohol becomes, in cooling, a solid crystalline mass. It is precipitated from its solution in alcohol by water, without acquiring any additional weight.

It is soluble in olive oil, and in oil of turpentine.

It does not combine either with an aqueous solution of potash or ammonia; nor is it sensibly affected by contact with ammoniacal gas.

Soluble in acetic and in oxalic acid, to each of which it communicates a clear pink colour. A saturated hot acetic solution becomes a solid crystalline mass in cooling.

It blackens sulphuric acid when boiled in it; the addition of water to the mixture having no other effect than to dilute the colour: neither does any precipitation take place upon saturating the acid with ammonia.

Sparingly soluble in hot muriatic acid, to which it communicates a purplish pink colour.

When boiled in nitric acid, it both decomposes the acid, and is itself altered in its composition; and, in cooling, is abundantly deposited in short acicular crystals aggregated in stelliform groups. These crystals pressed between folds of unsized paper, in order to separate the adhering acid, and then exposed to heat, are readily melted: in cooling, the melted mass shows evident traces of acicular crystallization, and the crystals are of a yellow colour. This yellow substance is readily inflamed, burns with a bright flame, emits much smoke, and leaves a considerable residuum of carbon.

Of all the characters of the white concrete substance described in this section, its ready disposition to crystallize is perhaps the most remarkable.

If thrown into a red hot crucible, a dense white vapour arises from it; which being received into a bell glass placed over the crucible, is condensed round the lower part of the glass in the form of a white powder; but in the upper and cooler part of the glass distinctly crystalline plates are formed, of a beautiful silvery lustre.

A similar and equally beautiful crystallization may be obtained by boiling this substance in water, in a glass matrass having a long

long neck; in the upper part of which crystals will be formed, and deposited during the boiling.

If exposed to a degree of heat not more than sufficient to melt it under a bell glass, the vapour that rises from it crystallizes before it reaches the surface of the glass, and flies about the interior with exactly the appearance of a shower of minute particles of snow-

If a piece of cotton twine be coiled up like the wick of a candle, and after having been dipped in this substance while melted be set on fire for a second or two, and then blown out, the vapour will soon begin to crystallize round the wick in very distinct thin transparent laminæ.

This experiment affords one mark of distinction between this substance and benzoic acid, and also between it and camphor: for, under similar circumstances, benzoic acid crystallizes in acicular crystals, which are often grouped in a stelliform manner; and camphor crystallizes, or is rather congealed, in globular particles having a stalagmitic appearance.

The most usual crystalline form of this substance is a rhombic plate, of which the greater angle appears to be from 100° to 105°: crystals at least of that form I have repeatedly obtained from its solutions in water, in alcohol, in acetic acid, in the yellow oil described in the last section; and lastly, by melting and very slowly cooling the substance itself. Sometimes several of these plates are variously grouped together; sometimes a single plate intersects another plate at nearly right angles, so that in some points of view the compound crystal appears simply cruciform. The only distinct modifications I have observed of the common form are a rhomboidal plate, which is very nearly rectangular; and an hexagonal plate: the latter variety may be easily traced from the rhombic plate by the incomplete development of the smaller angles of the usual rhomb.

The following process has been found most successful in illustrating the crystallization of this substance:

If 25 grains of it be dissolved by the assistance of heat in half a fluid ounce of alcohol, and the solution be cooled slowly in a glass matrass, it will begin to crystallize when nearly cool; and the matrass being placed between the eye aad a tolerably strong light, numerous transparent rhombic crystals will be visible; some of them reflecting from their whole surface a green colour; others, a blue; or a red; or some other of the prismatic colours.

With respect to the elementary constitution of this substance I am not enabled to give any satisfactory information; but it is evident that it contains a very great proportion of carbon. A small quantity of it was passed in the state of vapour through peroxide of copper heated to redness, and the only gaseous product

duct was carbonic acid: whether any water were formed, I could not ascertain.

It cannot be irrelevant to the object of this paper to state, that the white concrete substance which I have been describing, has twice been observed by me in the form of minute crystals, which beautifully reflected the prismatic colours, in the neck of an earthen retort in which animal matter had been submitted to destructive distillation.

Properties of the yellow Farina.

From the minute quantity of this substance which I was capable of obtaining, I could only ascertain one or two of its properties. It is soluble in alcohol, and forms a solution of a bright yellow colour: and it is precipitable from the solution, by the addition of water, in the form of a yellow powder, which remains permanently suspended in the mixture.

When heated, it melts into a substance of the consistence of a soft tough gum of a deep reddish brown colour.

Of the four several substances which result from the distillation of the black liquid described in the former part of this paper, it is probable that the water and the yellow farina are the only real products, and that the others are mere educts of that distillation: for, with respect to the water, its proportion is variable according to the greater or less degree of rapidity with which the distillation is conducted; and if it were present as water in the black liquid, there is reason to believe it would be found supernatant on its surface, after having remained still for some time. The essential liquid oil, and the white concrete substance, which pass over during the distillation, are probably contained originally in that thin portion of the black liquid which may be filtered through unsized paper; for the odour of this filtered portion closely resembles that of the oil; and the oil, by exposure to light, frequently becomes of a darker and darker shade, so as at last to be nearly of a deep brown colour; and with respect to the white concrete substance, this was not only found crystallized in that part of the original apparatus where the black liquid was condensed, but has been obtained from that liquid by simple evaporation of it at the common temperature of the atmosphere.

The yellow farina is probably produced fron the tar which is contained in the proportion of about one-fourth in the black liquid; for it does not make its appearance till towards the end of the distillation; when the more volatile substances have ceased to pass over, and the heat has been increased to the utmost: and if common coal tar be exposed to a low red heat, it will be found,

found, that when the tar has been nearly evaporated, this yellow farina will begin to pass off.

It remains for me to propose a name for the white concrete substance which has been described in this paper: and, unless a more appropriate term should be suggested by others, I would propose to call it Naphthaline.

III. *Reply to the "Apology for the Postscript on the Refractions" in No.* 24 *of The Quarterly Journal of Science. By* James Ivory, *M.A. F.R.S.*

To Dr. Tilloch.

Sir, — I have to request the favour of your inserting the following observations in reply to an article that has appeared in the last Quarterly Journal of Science. I shall take no notice of what is merely personal; but it would not be right to allow a writing so entirely calculated to mislead, to go before the public without making some attempt to enable it to judge of the merits of the case.

Although drawn up with some art and great apparent confidence, the article, in fact, leaves the observations I wrote on the new method of computing the refractions just in the same predicament they would be, if no such *apology* had been published.

I found that the series, or the development of the density of the air in terms of the refraction, was not sufficiently convergent to be of use. Does the author contradict this? He does not: on the contrary he allows it, by flying off to a different and more laborious method of computation, which has nothing to do with the construction of the table in the Nautical Almanack, the only point I proposed to examine, and the only point about which it is worth while to bestow a thought.

The method he employs consists in considering the variable quantities in the several stages of their increase, and computing their successive values by repeated operations. It is a method resorted to when all others fail. Recourse is had to it here from the want of convergency of the series first contemplated, and by which his table is constructed, with the hope, no doubt, of rescuing his mode of calculation from the reproach of a total failure.

The methods of calculation proposed by Dr. Young are not new, although he may be the only mathematician that has applied them to the problem of the refractions. They are the first that occurred in the progress of the integral calculus. Would it not

not therefore have been better to refer to some work of undoubted reputation with the public, than to have attempted to explain them by calculations, of which it cannot be said that any one result is accurate? But in this manner his readers would have been better able to judge of his consistency and fairness of arguing, when they found him affirming gravely that there is no want of convergency of the series, at the very time the default of convergency obliges him to employ subsidiary expedients.

By taking the whole values of the variable quantities at two intervals, he seems to have considerably diminished the error arising from the want of convergency of the series. But, how many intervals must be taken in order to exhaust it completely? We thus fall upon the same discussions agitated from the origin of the science. At any rate it appears necessary that he push his calculations up to the mark of truth, at least in some one instance, before the methods he recommends can be fairly compared with those usually followed. But, however this be, it must not be forgotten that the method of calculating by intervals, has nothing to do with the construction of the table in the Nautical Almanack.

The formula used in the construction of the table contains four terms; and the horizontal refraction in the table, is immediately found by solving the proper equation. But when we take a case of real theory; that is, one proceeding upon a given hypothesis of density, by which means the coefficients of the series are taken out of the clutches of the computer, and are derived solely from the nature of the case; then six terms of the series, not to say four, are totally inadequate for finding the refraction with the requisite exactness. What is the reason of this? Is it not that, in the one case, the coefficients are so adjusted as to bring out the desired result; while, in the other case, the expectation of the computer is balked, because the modelling of the series is placed out of his power?

The coefficient of the first of the four terms is unavoidably determined by the nature of the case, or by the differential equation: the other three are empirical. Nor will much be abated from this, if it be allowed that some assistance has been derived from a small exertion of the reasoning faculty in fixing the form of the coefficients, while their quantity is obtained entirely by a tentative method aiming at given results. Nothing in the apology is contrary to what is here advanced. It is admitted that the formula is partly empirical, and we are referred to Euler's Lunar Theory, as a parallel case. This instance is not very much to the point: for although the immensity of the calculations, and the impracticability of performing them, made it necessary to seek from observation what could not be found by theory, yet this

this must be considered as an imperfection and a blemish, if we may be allowed to use such words in speaking of a matter that so highly concerned the benefit of mankind. A few years ago, the Academy of Sciences proposed, for their prize-question, the Construction of Lunar Tables by Theory alone, the fortunate competitors being M. Damoiseau and MM. Plana and Carlini. But, in the case of the refractions, we are desired to hold a retrograde course, and are required to re-compute by an empirical formula the very same numbers already calculated by theory.

The author of the Apology misquotes my words, and slurs over the question of the identity of his table with that of the French. It is not enough to say that they agree in all ordinary cases; for there is no difference between them in the mean refractions. This is a fact of which any one may satisfy himself by reducing both tables to bar. 30, or both to bar. 29 93, the mean temperature being the same in both cases. The slight differences that occur will generally be found less than the discrepancies arising in solving over again the equations of the new method.

As there is no particular hypothesis of density adopted, the theory of the formula, if there be any, can be nothing but the general consideration that the density of the air, being a function of the refraction, may be developed in a series of the powers of that quantity. It therefore became necessary to prove not only that the series converged in every possible hypothesis of density, but that it converged so fast as to permit the rejecting of all the terms after the four first. Now this is not only not done, but it is not true.

But, it may be asked, how then does it happen that the formula represents the French mean refractions so exactly? Now even this question may, I think, be answered in a satisfactory manner. By adopting the hypothesis of a density decreasing uniformly, we obtain an exact solution of the problem of refractions in the form of an equation containing the two first powers of the quantity sought. The rules of Bradley, Mayer, &c. are all equivalent to the solution of a quadratic equation*. In their original form these rules can be applied only to compute the refractions at altitudes greater than 12 , or 14°; nearer the horizon they diverge from the truth. But if we relax from the strictly theoretical quantities, and determine the coefficients so as to represent the refractions at the horizon and at 45° from the zenith, we obtain empirical formulæ that apply with considerable exactness even at low altitudes. Now if to the two terms of such a formula, two more be added, so as to have three terms with indeterminate coefficients, a great latitude of calculation

* Kramp, Ref. Ast., p. 164.

will

will be acquired; and we may so determine the arbitrary quantities as greatly to diminish, and even almost to annihilate, the differences between the formula and observation, or between the formula and a given table of refractions. And this, I conceive, is a just and sufficient account of the coincidence between the tables of mean refraction in the Nautical Almanack and the *Connaissance des Tems*.

Suppose Dr. Young's formula with literal coefficients was given to each of two computers, one in London and one in Paris; and they were directed to determine the numerical values so as to represent the French table: it is by no means clear that both would hit upon the same numbers for the coefficients. It will not appear improbable to any one who has attended to the variety of numerical formulæ for calculating the refractions *, that the result of such an experiment might be, two different formulæ equally representing the prescribed table.

It will not, I hope, be inferred from any thing that has been said, that an empirical table of refractions is supposed to be of little value. It can indeed have no value at all unless it have a proper foundation of its own, which can only be the case when it is constructed from an extensive series of observations made in every diversity of circumstances. A table, however constructed, that is a mere copy of another, can have no authority which the original does not possess.

Upon the method of allowing for the variations of the barometer and thermometer, I made no observations. It would be very difficult to prove in a strict manner either its correctness or incorrectness. Besides, it is independent of the new method for the mean refractions, which alone I undertook to examine. This independence of the two methods arises from the empiricism of the formula. For had the formula been theoretical, the coefficients, instead of being numbers, would have contained the quantities that vary with the state of the atmosphere; and one expression would have served, as ought to be the case, both for the mean refractions, and the mutations they undergo by the barometrical and thermometrical changes. The safest way to deal with this part of the table, is to compare it with some other table of at least equal authority. That of Dr. Brinkley will answer best, because the two tables agree in having the same mean horizontal refraction. Thus, for bar. 30 and ther. 50°, the horizontal refraction is,

Dr. Brinkley	33′ 50″
N. A.	33 51.

Now, suppose a change of temperature of 18°, and compute

* De Lambre's Astronomy, vol. i. chap. 13.

 the

the refractions for the zenith-distances 90° and 89°, bar. 30 and ther. 32° :

	Z. D. 90°	Z. D. 89°
Dr. Brinkley	36′ 51″	26′ 2″
N. A.	36 17	25 50
Diff.	34	12

And such differences must always occur, unless some general principles be adopted, or some general mode of solution can be found out.

Allow me, sir, before I conclude, to say a word about the "Concessions" in your Mag. for last November. It is not easy to state distinctly what is conceded and what is withheld. The balance seems to be poised with a very even hand, between the concessions to be made, and the tone of authority to be kept up. If I might presume to give an opinion, I would say that the only error I am now charged with, relates to my number ·00419 which he makes ·00416; amounting to $\frac{3}{100000}$ of an inch, if we speak absolutely; or, relatively as my antagonist takes it, to $\frac{1}{140}$ of the existing quantity. There is some refinement in this way of reckoning; for the less the quantity, the greater the error. I am sure there is nobody who has attended to the controversy, but will allow that the chance of an error in his number is greatly in my favour; but the whole difference is so very little, and he has already descanted upon it so amply, that it would be a pity to add another word upon the subject. Besides, we shall soon have a table of surpassing accuracy; when he has spent his money in hiring a host of computers to complete his lucubrations.

I have some consolation, sir, in thinking that the discussions, in your work, on the subject of the refractions, will be found not altogether unimportant or uninteresting to the astronomer. I allude to the general view of the problem in your Magazine for May last, and to the formulæ for the mean refractions in the same, and the following number; to the observations made on the hypothesis of Cassini; and particularly to the remarks on Mayer's formula in the Magazine for November. Since writing that article I have looked into the *Fundamenta Astronomiæ* of Professor Bessel, who is the only author I have met with that does justice to the astronomer of Göttingen. In speaking of the correction for the thermometer, he thus expresses himself, p. 26,

"Ceterum in hoc quoque capite non æquales solum, verum etiam posteriores astronomos antecessit Tobias Mayer, in refractionis formula rectius adhibens thermometri correctionem; utrum observationes an theoria eum huc perduxerint latet: sed confitendum est, correctionem illam postea inutilem atque falsam judicatam ejusque auctorem vituperatum esse quod eam calculis inseruerit.

inseruerit. Etsi subtilis theoretica thermometri correctionis determinatio non prorsus congruit cum Mayeri hujus rei tractandæ ratione: tamen, si eam recipissent astronomi, maximam partem evitassent errorum, quos gignet refractio, aeris densitati in observatoris loco aut refrangendi facultati tota proportionalis posita."

It now remains that I thank you, sir, for your attention to my communications, and that I express my regret you were troubled with the short letter in your last Number: but I was not then aware that a public man, upon a public question, would descend to personal abuse, even if he found himself without good arguments to urge in his defence.

I am, sir, &c.

Jan. 7, 1822. JAMES IVORY.

IV. *On Short-hand Writing.* *By* HENRY UPINGTON, *Esq.*

To Dr. Tilloch.

Blair's Hill, Cork, Nov. 5, 1821.

DEAR SIR, — GIVE me leave to occupy your attention for a short time, upon a subject which, although in itself not a branch of philosophy or literature, must, if successfully cultivated, be acknowledged as a valuable acquisition by every one who is desirous of occasionally taking down the heads of a discourse, or who devotes a considerable portion of his life either to the transcribing of the works of others, or to original composition.

You will very easily perceive, sir, by this prefatory observation, that I should willingly realize, as far as in my power, the suggestion of Mr. Locke, by putting every gentleman in possession of the most expeditious method of short-writing compatible with perspicuity and ordinary muscular execution. This is most certainly my intention; and if I should be so fortunate as to enable the literary part of my countrymen to save, in the course of every day, even one or two hours which must otherwise be devoted to manual drudgery, I shall feel myself most amply recompensed.

The prominent objection of the most intelligent persons with whom I have conversed, to the cultivation of short-hand as generally practised, is in my opinion extremely rational. They insist that even *years* are necessary to execute with sufficient ease the various crabbed angles, and consequent difficult combinations dependent upon the four different positions, left, right, perpendicular and horizontal, as thus / \ | —: and that until an absolutely automatical command of these be obtained, even the intellectual Note-taker or Reporter who uses short-hand is very

very little superior to a mere operating mechanic, for ever attending to his fingers, but incapable of exercising his head, whether for the necessary rejection of tautology or the judicious condensation of the subject.

In confirmation of the justness of this objection I may add, if necessary, my own experience. These five-and-twenty years I have been in the habit of using short-hand for my private purposes: and, although I had very early the good fortune to obtain for myself what practical short-hand writers would call a *superior* method, as embracing the principal conveniences and rejecting the principal inconveniences of the methods of Dr. Byron, Mr. Gurney, Mr. Taylor, and Dr. Mavor, while at the same time it was somewhat swifter than all; yet so opposite are the muscular motions, even on this plan, to those to which I am every day accustomed in common writing, that after a lapse of two or three weeks without using short-hand, I am compelled to repractise it for half an hour at least, in order to attain my previous facility. As to the taking down a public discourse, *verbatim*, I know not what extraordinary application may have accomplished; but in candour I must acknowledge my incapacity. Although a tolerably quick writer, I have never at any time been able to take down in a desirably copious manner, even the substance of a sermon: certain difficult combinations never failed to obtrude themselves—my attention was distracted—and I lost the speaker.

After having thus stated one formidable argument against the study of short-writing by the gentleman who does not mean to use it as a profession—to which argument may be added, the undeniable difficulty of *reading* it; you will naturally be desirous to learn, what method I can propose that shall operate, in any material degree, towards the removal of such rational objections. My intended answer is the result of experience, not of theory; and therefore I shall not hesitate to make it. It is briefly this:

First, That the simplest and most easily executed scheme of *consonants* be contrived—in which scheme, all characters descending in straight lines towards the *right* shall be rejected, unless in the middle or ending of a word when *preceded*, and at all times, even in the beginning of a word, unless *followed* by an ascending stroke, as thus  or thus : and by which scheme no definite angle, nor even perpendicular line unless when alone, shall ever be required; while, for perspicuity, all the common stops may without confusion be introduced.

Secondly, That with regard to *vowels*—the MASORETIC method of writing the Hebrew language be almost exactly adopted: by which I mean—that every word shall be expressed by its consonants

sonants alone—the simplest vowel characters devisable being subsequently applied, whether in the beginning, the middle or the end of words, as the writer shall consider them expedient.

Thirdly, As to the *reading* of an extensive manuscript in which these or any other short-hand characters are solely used, with satisfactory readiness, at a glance, when the subject itself is altogether or very nearly forgotten by the writer: although some of our stenographic bookmakers may insist on the facility of so doing, after a few months or even weeks of application; yet I cannot by any means hold out so fallacious an expectation. On the contrary, *years* are indispensable: nor is it likely that any one gentleman in a thousand (I speak not of the professional stenographist) shall ever attain this ultimate object by any other process than that which I have seen successfully adopted;—the intermixing, with his common writing, the pronouns, auxiliary verbs, conjunctions and other minor parts of speech expressed in short-hand; and proceeding from thence, step by step, slowly yet systematically, to encroach upon his long-hand.

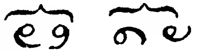
Lastly, With respect to the possibility of ever following a speaker, *verbatim*, by the apparently slow method I have suggested—the sequel shall determine. In the mean time let the literary gentleman reflect, that even if no other object be attainable than that of expressing all our ordinary words in short-hand, with about four times his usual expedition, by which means more than one third of his whole time shall, in a few weeks, be saved;—let him, I say, reflect, that these few weeks devoted to such an attainment will have been very judiciously employed.

Were I in the least disposed, tediously to engross the pages of your Journal, and consequently to exhaust the patience of its readers, I should enter into a long detail of the *history* of short-writing taken from the voluminous works of our very learned English authors upon this *art*, to which, not satisfied with the generally understood name of *Short hand*, they have assigned the very lofty appellations of brachygraphy, cryptography, stenography, tachygraphy, zeitography, semigraphy, or "the world's rarity," with a numerous train of *etceteras* all dignified by the title of "systems:" I should literally *carry* my reader to China; from thence to Egypt, and from Egypt to Greece and Rome—where I should leave him no wiser than I found him, unless it be deemed worthy of our notice that, in addition to the methods of abbreviation practised by the Romans, and of which even Ainsworth's Dictionary has given us most copious specimens, there were also used by some of their *notarii*, certain arbitrary characters called *notæ* in opposition to *literæ*, by which not only certain terminations but several thousand Latin words were expeditiously expressed.

From

From Rome I should travel to England, and there introduce my reader to the unparalleled Timothy Bright, who lived in the reign of Elizabeth, and who, as we are informed, was the first inventor of a stenographic Alphabet, which he dedicated to that Queen. I should even rally my countrymen upon their various whimsies styled *improvements* of the art; such as the writing of whole sentences without taking off the pen—or the creation of three or even five real or imaginary lines called " places," which, like our musical stave, shall metamorphose one letter into another at pleasure, or even dispense altogether with certain *commencing* letters, through the agency of the *name* of that place upon which the *second* letter shall be made. Neither should I hesitate to set forth the pedantic introduction, called " Invention," of a whole host of Latin prepositions, such as *omni*, *post*, and *preter*—ill suited to the genius of our language, and calculated neither for perspicuity, nor, on the great average of syllables, even for brevity itself. I should perhaps also state the various important controversies of our very learned cryptographists—whether, in the writing of any individual *word*, the hand should or should not be ever lifted at all: but as I cannot ensure to myself a patient reading, by the unlearned world, of such enlightened topics, I shall pass on in my own way with the subject, and lay before you what many will consider a very usefu though perhaps not a very amusing *Table* of all the short-hand characters deserving the name of *alphabetical.*

TABLE OF ALPHABETICAL SHORT-HAND CHARACTERS, *arranged in the order of Simplicity*, i. e. *commencing with the most simple and regularly proceeding to the most complex.*

1st. Right lines	/ / \ | —	=5
2d. Curves [any thing approaching semicircles]	( ) ⌒ ⌣	=4
3d. Right lines beginning with a curve or hook		=5
4th. Right lines beginning with a loop		=5
5th. Curves (nearly semicircles) beginning with a loop ..		=2
	Reject, as explained below ..	3
	Remain	18

Note.

Note. As it may appear rather strange to those who are unacquainted with short-hand, why the two first characters of the first series are apparently similar; it may not be impertinent to observe, that almost all our stenographists have, by a very simple contrivance, rendered them virtually distinct—the one being an ascending stroke and connected with the following letter thus , the other descending and connected thus .

Note also, that the first four characters of the third series, as well as the third and fourth characters of the fourth series, are ineligible for *general* purposes. If we add to this the necessity for junction, or at least the extreme convenience of appropriating two *hooked* characters (that is, our choice of either) to an individual letter; and the similar necessity of appropriating two *looped* characters, in like manner, as indicated by the respective *braces* set over those characters in the table—we shall find the number of our truly alphabetical letters reduced to *eighteen*.

Now with regard to the utility of this table, is it not obviously a material guidance for the construction of an alphabet?—and who, without a thorough knowledge of all the existing characters, together with a knowledge of the ease or difficulty of their formation, their comparative swiftness, their eligibility for junction, their distinctness when swiftly written, or their tendency to promote or injure lineality, shall pretend to lay down a rational scheme of short-hand? But even this knowledge is insufficient. The ratio of occurrence of all the *consonants* of the language for which a short-hand alphabet is intended, must be tolerably well ascertained; the incipient ones, or those which first present themselves in every word, as the *n* in *on*, *no*, *never*, being distinguished from the subsequently occurring consonants in every word [I shall call them *subsequents*], as the *v* and *r* in the last-mentioned dissyllable *never*, or the *grd* in the word *regard*. Here I must request of the intelligent reader already conversant in the principles of short-hand, that he will not censure my prolixity. This paper is intended merely for the information of those gentlemen who may wish to obtain a mastery of this art—but whose valuable time may otherwise be sacrificed to the ignorance or cunning of an empiric. Nor is this observation uncalled for: more than one gentleman of my acquaintance has reason to regret his unprofitable labour.

The difficulty, or rather the trouble, of forming such a "ratio of occurrence" as that of which I have just spoken, is indeed so great, that were it not for the indefatigable exertions of a literary friend, I should in all probability have never obtained so valuable a document. Several weeks were devoted by him to the scrutiny. Parliamentary and forensic speeches, sermons, philosophical

lectures, polite literary correspondence—*all* were separately explored; and an average was taken of the whole.

This very useful table, formed from upwards of one hundred thousand letters, was constituted thus; the highest number, N, being reduced to 1000 as the standard.

Table of the relative occurrence of the various Consonants [quiescent ones not reckoned] of English classical Composition—whether incipient consonants or subsequents: commencing or incipient y *(together with the double letters* ch, sh, th, wh, *wherever found) being considered among the number of those consonants; and also the treble letter* thr, *whether a vowel be interposed or not between the* h *and* r. STR *was too unimportant to introduce.*

	Alone; or incipient.	Subsequent.	Totals.	
B	154	58	212	
hard C	..	..	..	expressed by K.
soft C	..	..	..	.. .. S.
D	103	359	462	
F	198	85	283	
G	33	114	147	
H	77	..	77.	Subsequent H, unconnected with C, S, T or W, is considered an *aspirate*.
J	5	5	10	
K	119	113	232	
L	77	278	355	
M	130	139	269	
N	361	639	1000	
P	136	109	245	
Q	3	9	12	
R	105	574	679	
S	255	507	762	
T	236	581	817	
V	73	81	154	
W	122	7	129	
X	14	13	27	
Y	52	..	52	Almost all occasioned by the 2d person, *you, ye, your.* Independently of these, it occurred but thrice.
Z	0	2	2.	This letter Z does not occur *once* on the present scale, as an incipient.

Ch

	Alone; or incipient.	Subsequent.	Totals.	
Ch	2	43	45.	Ch was represented by K when so sounded, as in "*chymist.*"
Sh	19	11	30	
Th	282	32	314	
Wh	52	2	54	
Thr	28	21	49	
	2636	3782	6418	

Arrangement *in the order of frequency.*

N, T, S, R, D, L, Th, F, M, P, K, B, V, G, W, H, Wh, Y, Th
Ch, Sh, X, Q, J, Z.

Note. The average number of *words* attachable to the foregoing table; or, in other terms, the average number of words expressed by 6418 short-hand consonants, is 2743, which is almost fractionally equal to $2\frac{1}{3}$* such consonants for every individual word. *Arbitraries,* it is true, may provide for *some* of these; but comparatively for so few that this table must serve, with sufficient accuracy, as the basis of any intended calculation.

Suppose that, for example's sake, I were to start a question, Let the descending oblique right line / be excluded as an *independent* letter; and the writer be privileged to exchange, when desirable, the perpendicular line | for the foregoing oblique one .... thus obviating many difficult angles: What loss, then, shall be sustained by adopting, for the letter L, the looped character in place of the relinquished line /; taking it for granted that looped characters, except in the beginning of words, are nearly equal to simples †;—but that in the beginning of words, or when alone, a loss equal to $1\frac{1}{2}$ right line is sustained by every looped character?

In my opinion, this question may be solved by the judicious application of our table, thus:

Let the aggregate of our consonants, 6418, be rated on the average, as equal to $1\frac{1}{2}$ right line each [near enough for our purpose]:

* This average does not hold good with *vulgar* composition, which almost constantly takes but *two* short-hand characters, or thereabout, to every word.

† When the license of turning the loop in the requisite direction is given to the writer—as thus in place of .

purpose]: there shall result from this a number equal to	9627 right lines.
To which add the lines formed *in air*, by lifting the hand between each word ..	2742
Add also the supposed number of vowels (including A and I when necessary) which cannot, without too much risk of illegibility, be dispensed with	= 350
And add likewise; loss by lifting the hand *in air* to form those vowels ..	350
Total	= 13069 right lines,

or, in round numbers, 13,000.

Now, if in writing a number = 13,000 right lines, the letter L, as an *incipient*, shall occur but 77 times, producing a loss = 115 right lines; the aggregated loss is evidently but the 113th part of the whole, or very nearly equal to half a minute in an hour.

Pursuing the same mode of calculation, incipient K, too, if expressed by in place of \, will yield a loss of almost exactly one minute in an hour;—and this sacrifice, as well as that arising from the looped L (supposing even the aggregate loss increased by one-half, in consequence of the disadvantage of these characters when intermediate or final), I shall make to a certain extent* in the formation of my alphabet.

[To be continued.]

* The plan of *prepositives* which I mean, by and by, to suggest, will almost wholly remove the *incipient* disadvantage.

V. *Ephemeris of the newly-discovered Planets for their several Oppositions in* 1822: *calculated by* S. Groombridge, *Esq. F.R.S., and presented by him to the Astronomical Society of London.*

Pallas and Ceres being near the aphelion, it is doubtful whether they will be visible at the opposition; particularly the former, by reason of the great excentricity of its orbit. It was therefore unnecessary to compute their places to the stationary points. The orbit of *Vesta* having been found from later observations less than heretofore computed, the mean longitude in the tables of Mr. P. Daussy (published in the *Connaissance des Tems* 1820) has become nearly 20 minutes in arrear.

Vesta.

VESTA.

1822.	Æ	Dec. S.	1822.	Æ	Dec. S.
	h ′ ″	° ′		h ′ ″	° ′
April 19	18. 0.53	17. 9	June 21	17.31.57	19.24
22	2.14	9½	24	28.57	36
25	3.17	11	27	26. 4	48½
28	4. 2	12½	30	23.20	20. 0½
May 1	4.29	14½	July 3	20.47	13
4	4.38	17	6	18.27	25½
7	4.29	20	9	16.21	38
10	4. 2	24	12	14.30	50½
13	3.15	28½	15	12.56	21. 3
16	2.11	34	18	11.40	16
19	0.47	39½	21	10.43	28½
22	17.59. 7	46	24	10. 4	41
25	57.10	53½	27	9.43	53½
28	54.58	18. 1½	30	9.41	22. 6
31	52.33	10	Aug. 2	9.59	18½
June 3	49.56	19½	5	10.35	31
6	47. 9	29	8	11.29	43½
9	44.12	39½	11	12.39	55½
12	41.11	50	14	14. 6	23. 7½
15	38. 7	19. 1	17	15.51	19½
18	35. 0	12½	20	17.51	31

April 30. In perihelio.
May 4. Retrograde in long.
June 16. Opposition.
July 28. Direct in long.
August 13. In descending node.

PALLAS.

1822.	AR	Dec. N.
	h ′ ″	° ′
July 13	20.43.52	16.49½
16	41.40	41
19	39.26	29½
22	37.11	15
25	34.56	15.58½
28	32.40	40½
31	30.20	20
Aug. 3	27.58	14.58
6	25.33	33½
9	23.10	7½
12	20.55	13.40
15	18.47	10
18	16.46	12.39
21	14.51	6
24	13. 2	11.32
27	11.22	10.57½
30	9.50	23
Sept. 2	8.27	9.48

June 18. In aphelio.
August 4. Opposition.

CERES.

1822.	AR	Dec. S.
	h ′ ″	° ′
Aug. 1	22.42.45	23.21
4	40.53	42½
7	38.52	24. 4
10	36.45	25
13	34.31	45½
16	32.12	25. 5
19	29.43	23½
22	27. 6	42
25	24.30	59½
28	21.57	26.16
31	19.24	31
Sept. 3	16.52	43
6	14.22	52
9	11.56	58½
12	9.40	27. 5
15	7.32	11
18	5.32	16
21	3.41	20

July 25. In aphelio.
August 22. Opposition.

JUNO.

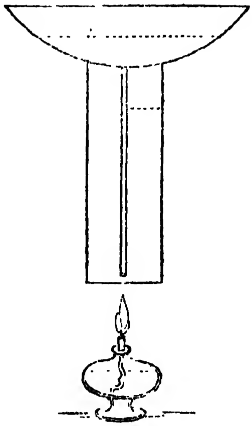
JUNO.

1822.	AR	Dec. N.	1823.	AR	Dec. N.
	h ′ ″	° ′		h ′ ″	° ′
Nov. 13	8. 1.48	2.56	Jan. 12	7.44.54	1.39½
16	3.32	35	15	42. 5	2. 1
19	4.57	14½	18	39.29	24
22	6. 6	1.55½	21	36.53	48½
25	6.59	38	24	34.22	3.15½
28	7.35	21½	27	32. 0	43
Dec. 1	7.51	7	30	29.48	4.12
4	7.50	0.54	Feb. 2	27.49	41½
7	7.32	42½	5	26. 2	5.11½
10	6.56	34	8	24.27	42
13	6. 1	27	11	23. 9	6.12½
16	4.51	22½	14	22. 8	43
19	3.24	21	17	21.22	7.13
22	1.43	21½	20	20.52	42½
25	7.59.46	25	23	20.40	8.11½
28	57.39	30	26	20.44	39½
31	55.20	39	March 1	21. 5	9. 7
1823.			4	21.41	33
Jan. 3	52.51	51	7	22.30	58
6	50.17	1. 4½	10	23.36	10.22½
9	47.38	20½	13	25. 0	45

1822. July 30. — In perihelio.
December 7. — Retrograde in long.
1823. January 17. — Opposition.
February 27. — Direct in long.

VI. *On the boiling Springs of Iceland.* *By Mr.* JOHN MURRAY.*

In reading the description of the boiling springs or geysers of Iceland, as given to us by Stanley, Hooker, Mackenzie, and by Henderson, I found it difficult to account for the *intermission* of the jets, supposing the subterranean fire to continue *uniform* in temperature.



I caused an apparatus to be constructed, which tended to explain the phænomena of the intermission of the jet and recession from the basin into the central pipe. A section of that simple apparatus is on the margin, and its phænomena clearly and satisfactorily prove that the circumstances adverted to, are ascribable to the *cooling* of the water from the united influences of *radiation* and *evaporation*. Radiation, from the surface of the water in the basin into which it rises; and Evaporation, from that dispersed into the atmosphere in the play of the geyser.

The apparatus consists of a cylindrical tin case surmounted by a concave basin, into which the water rises through a central pipe (representative of the siliceous stalactitic pipe obtaining in the geysers, the consequence of deposition of siliceous matter from the water containing silica and soda in solution), and which descends nearly to the bottom of the cylinder.

The apparatus being supplied with water, and a spirit lamp introduced, the water will, in a short time, be perceived slowly ascending into the basin. The steam finally bursts through the water and forms an irregular jet; and so soon as the water is cooled by the causes adverted to, it retires from the basin into the pipe, and the same phænomena are reiterated at intervals. The experiment is a very beautiful one, and always gratifying.

Dr. Henderson has stated a curious fact with respect to these wondrous phænomena; and though it has been rudely questioned, it is one, surely, that may be conceived a necessary result. I advert to the circumstance of the play of the geysers *being more promptly determined by casting stones into the pipe.* This is easily explained by supposing the pipe at its lower extremity *curved* (a phænomenon which I myself have witnessed

* This paper was transmitted to the Wernerian Society of Edinburgh.

witnessed in some of the caverns of Derbyshire, where the ends of the stalactites depending from the roof are *hooked*, or curved upwards). Now on this supposition, if a stone or stones were thrown in, either wholly or partially to blockade that orifice, the steam would be thereby confined, and sooner be raised to a maximum, because the water is then prevented from its slow and gradual ascent into the basin, and thus diminishing the amount of the elasticity of the steam; whereas, in common circumstances the steam sallies forth at intervals through the water, before it obtains the force necessary to the propulsion of the jet into the atmosphere.

VII. *On a new Compound of Chlorine and Carbon. By* Richard Phillips, *F.R.S. E. F.L.S. M.G.S., &c., and* Michael Faraday, *Chemical Assistant in the Royal Institution. Communicated by Sir* Humphry Davy, *Bart. P.R.S.**

M. Julin, of Abo, in Finland, is proprietor of a manufactory, in which nitric acid is prepared by distilling calcined sulphate of iron with crude nitre in iron retorts, and collecting the products in receivers connected by glass tubes, in the manner of Woulfe's apparatus. In this process he observed, that when a peculiar kind of calcined vitriol, obtained from the waters of the mine of Fahlun, and containing a small portion of pyrites, known in Sweden by the name of calcined aquafortis vitriol No. 3, was used, the first tube was lined with sulphur, and the second with fine white feathery crystals. These were in very small quantity, amounting only to a few grains from each distillation; but M. Julin, by degrees, collected a portion of it, and, having brought it to this country, inserted a short account of its properties in The Annals of Philosophy, vol. i. p. 216, to which a few observations were added by ourselves.

The following are the properties of this substance, as described by M. Julin. It is white; consists of small soft adhesive fibres; sinks slowly in water; is insoluble in it whether hot or cold; is tasteless; has a peculiar smell, somewhat resembling spermaceti; is not acted on by sulphuric, muriatic, or nitric acid, except that the latter by boiling on it gives traces of sulphuric acid; boiled with caustic potash, has a small portion of sulphur dissolved from it; dissolves in hot oil of turpentine, but most of it crystallizes in needles from the solution on cooling; dissolves in

* From the Transactions of the Royal Society for 1821, Part II.

boiling alcohol of ·816, but by far the greater part crystallizes on cooling; burns in the flame of a lamp with a greenish blue flame, giving a slight smell of chlorine gas; when heated, melting, boiling, and subliming at a temperature between 350° and 400°, and subliming slowly without melting at a heat of about 250°, forming long needles. Potassium burned with a vivid flame in its vapour in an open tube, and carbon was deposited; a solution made of the residuum, and saturated with nitric acid, gave a copious precipitate with nitrate of silver. M. Julin then remarks, that the small quantity he possessed, with want of leisure, prevented him from making any further experiments on it; and concludes, by comparing it with the chlorides of carbon that have lately been formed.

The small quantity of the substance which, by the kindness of M. Julin, we had at our disposal at that time, was insufficient to enable us satisfactorily to ascertain its nature. We found it mixed with free sulphur, and sulphate and muriate of ammonia. When purified, our first object, in consequence of M. Julin's suggestion, was to compare it with the per-chloride of carbon, but it was found entirely distinct from it in its properties.

Since M. Julin's return from the continent, he has very kindly placed some further portions of this substance at our disposal. We have therefore been enabled to continue our experiments, and have come to the very unexpected conclusion of its being another chloride of carbon, in addition to the two, an account of which has been published in the Transactions of the Royal Society for this year.

The substance, after being boiled in solution of potash, washed in water, dried and sublimed, formed beautiful acicular crystals, which appeared to Mr. W. Phillips to be four-sided prisms. They contained no sulphur, and, when dissolved in alcohol or ether, gave no traces of chlorine or muriates, by nitrate of silver. They burned in the air with a strong bright flame at a heat below redness, and agreed with the description given by M. Julin of the properties of the substance.

When heated moderately, it sublimed unaltered; but on passing a portion over rock crystal, heated to bright redness, in a green glass tube, it was decomposed, charcoal was deposited, and the gas, passed into solution of nitrate of silver, precipitated it, and proved to be chlorine.

A portion was repeatedly sublimed in a small retort filled with chlorine, which was made red hot in several places; it however underwent no change: but on cooling crystallized as at first. It was also exposed in the same gas to sun light for many days, but no change took place.

When

When raised in vapour over hot mercury, and detonated with excess of oxygen, a quantity of carbonic acid gas and chloride of mercury were produced. There was no change in the volume of gas used; and lime water being passed into it absorbed the carbonic gas, became turbid, and left a residuum of pure oxygen. Acetic acid being then added, to dissolve the carbonate of lime, the solution was tested for chlorine, which was readily found in it. When detonated with oxygen, the substance being in excess, there was expansion of volume, carbonic oxide, carbonic acid, and chloride of mercury being formed.

When phosphorus, iron, tin, &c. were heated to redness in its vapour over mercury, it was decomposed, chlorides of those substances being formed, and charcoal deposited; and M. Julin has shown that the same effect is produced by potassium.

Three grains of this substance were passed in vapour over pure peroxide of copper, heated to redness in a green glass tube: a very small portion passed undecomposed. The gas received over mercury equalled 5·7 cubic inches; it was carbonic acid gas. A small part of the oxide of copper was reduced, and portions of a crystalline body appeared within the tube, which, on examination, proved to be chloride of copper. Some of this was used in making experiments on its nature; but when that was ascertained, the remaining contents of the tube were dissolved in nitric acid, and precipitated by nitrate of silver: 6·1 grains of chloride of silver were obtained.

Two grains were passed over pure quick lime, raised to a red heat in a green glass tube. The moment the vapour came in contact with the hot lime, ignition took place, and the earth burned as long as the vapour passed over it. When cold, the tube was examined, and much charcoal found deposited at the spot where the ignition occurred. The contents of the tube were dissolved in nitric acid, and the filtered solution precipitated by nitrate of silver: 5·9 grains of chloride of silver were obtained.

These results afford us sufficient data from which to deduce the nature and composition of this body. All the experiments of decomposition indicate it to contain chlorine and carbon, and those with oxygen and the metals sufficiently prove the absence of hydrogen and oxygen. With regard to the proportions of the elements, three grains of the substance gave 5·7 cubic inches of carbonic acid gas, therefore two grains will give 3·8 cubic inches. One hundred cubic inches of carbonic acid gas weigh 46·47 grains, and contain 12·72 grains of carbon; and 3·8 cubic inches will therefore contain 0·483 grains of carbon. The two grains of the substance decomposed by heated lime gave 5·9 grains of chlo-

 ride

ride of silver, which, according to Dr. Wollaston's scale, equal 1·45 of chlorine; hence the two grains gave chlorine .. 1·45

chlorine	..	1·45
carbon	..	·483
		1·933

The loss here is 0·067, which is by no means important, when the small quantity of the substance and the nature of the experiments are considered.

As to the proportion of these two bodies to each other, if we consider chlorine as represented by 33·5, and carbon by 5·7, or with Dr. Wollaston by 44·1 and 7·5, then the 1·45 of chlorine would be equivalent to 0·2466 of carbon. This is the constitution of the fluid or proto-chloride of carbon; and if we double the 0·2466, the product 0·4932 approaches so near to the experimental result 0·483, that we do not hesitate to regard this compound as consisting of one portion of chlorine and two portions of carbon, or

chlorine	.. ..	44·1	33·5
carbon	.. ..	15	11·4

It is remarkable, that another of these compounds should be found so soon after the discovery of the two former chlorides of carbon. Its physical properties, and its chemical energies, are in every respect analogous to those of the former compounds; and its constitution increases the probability, that another chloride of carbon may be found, consisting of two portions of chlorine and one of carbon.

All the endeavours we have yet made to form the chloride of carbon now described, or to convert it into either of the other chlorides, have been unsuccessful. We expected that, when decomposed by heat, it would produce the proto-chloride with the liberation of carbon, as the perchloride does with the liberation of chlorine, but we have not yet been able to ascertain that point. We have only to offer as an apology for this and other imperfections in the present paper, the smallness of the quantity of this substance that we possessed.

VIII. *Table of the periodical Variation of the Star* Algol, *from February to December* 1822, *inclusive.*

To Dr. Tilloch.

Sir, — As I conceive every gentleman attached to astronomical pursuits is in the habit of seeing your excellent monthly publication, it is probable that the insertion of the following Table of the periodical variation of *Algol* may enable some of your readers to amuse themselves with the observation of that curious phænomenon. The table has been long since printed in Bode's Ephemeris

Ephemeris for 1822, and contains the period of the star's *least* magnitude, according to Paris time. It commences January 4, 1820, and continues to the end of the present year—that part, therefore, which remains unexpired, I now transmit. It is rather singular, that no one has already pointed out this circumstance to the public, as the Berlin Ephemeris, from being written in the German language, is not very generally circulated in this country.

Winterdyne, Jan. 19, 1822. W. M. M.

Table by Professor Wurm, *of Stuttgart, in mean Paris Time.*

1822.			H.	M.					H.	M.	
February	2	...	2	1	Morn.	September	5	...	3	14	M.
	4	...	10	50	Even.		8	...	0	3	
	7	...	7	39	Ev.		10	...	8	52	Ev.
	10	...	4	28			25	...	4	57	M.
	22	...	3	44	M.		28	...	1	46	
	25	...	0	33			30	...	10	35	Ev.
	27	...	9	21	Ev.	October	3	...	7	24	
March	14	...	5	26	M.		6	...	4	13	
	17	...	2	15			18	...	3	28	M.
	19	...	11	4	Ev.		21	...	0	17	
	22	...	8	53			23	...	9	6	Ev.
April	6	...	3	58	M.		26	...	5	55	
	9	...	0	47		November	7	...	5	11	M.
	11	...	9	36	Ev.		10	...	2	0	
	29	...	2	30	M.		12	...	10	49	Ev.
May	1	...	11	19	Ev.		15	...	7	38	
	22	...	1	2	M.		18	...	4	27	
	24	...	9	51	Ev.		30	...	3	43	M.
June	11	...	2	45	M.	December	3	...	0	32	
	13	...	11	34	Ev.		5	...	9	21	Ev.
July	1	...	4	27	M.		8	...	6	10	
	4	...	1	16			20	...	5	26	M.
	6	...	10	5	Ev.		23	...	2	15	
	24	...	2	59	M.		25	...	11	4	Ev.
	26	...	11	48	Ev.		28	...	7	53	
	29	...	8	37			31	...	4	42	
August	13	...	4	42	M.						
	16	...	1	31							
	18	...	10	20	Ev.						

IX. *True apparent Right Ascension of Dr.* Maskelyne's 36 *Stars for every Day in the Year* 1822, *at the Time of passing the Meridian of Greenwich. By the Rev.* J. Grooby.

The mean Right Ascensions are taken from Mr. Pond's Catalogue in the Nautical Almanac for 1823, and the Corrections from the Tables of M. Bessel. On those days where an asterisk is prefixed the Star passes twice, the Æ there given is that at the first passage. 1822.

1822.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran	Capella	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 53	H. M. 4 25	H. M. 5 3	H. M. 5 6	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M 7 30	H. M. 7 34	H M 9 18	H. M. 9 58	H. M. 11 39	H. M. 11 41	H. M. 13 15	H. M. 14 7
Feb.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s	s.	s.	s.	s.	s.	s.	s.	s.
1	4·98	10·29	0·15	44·73	35·73	1·01	5·00	34·36	20·07	16·56	1·21	27·52	52·65	55 51	40 0·65	27·50	51·10	34·21
2	97	27	14	72	71	0 99	4·99	35	06	56	21	53	66	52	67	52	13	24
3	96	26	13	71	70	98	98	35	06	56	21	53	67	54	70	55	16	28
4	96	25	11	69	68	97	97	34	05	56	21	53	68	55	73	57	19	31
5	95	23	10	68	67	96	96	33	05	56	21	53	69	57	75	59	22	34
6	94	22	09	67	65	95	95	32	04	56	21	53	70	58	78	61	25	37
7	93	21	08	66	64	94	94	32	04	56	21	53	71	60	80	64	28	40
8	93	19	06	65	62	92	92	31	03	56	21	53	72	61	82	66	31	43
9	92	18	04	63	60	91	91	30	02	55	20	53	72	62	84	68	34	46
10	92	16	03	62	58	89	89	29	01	55	20	52	73	63	86	70	36	49
11	91	15	01	60	56	88	88	28	00	55	20	52	74	64	88	72	39	52
12	91	14	00	59	54	87	87	27	19·99	54	19	52	74	65	90	74	42	55
13	90	13	52·59,99	58	53	85	85	26	98	54	19	51	75	66	92	76	44	58
14	90	11	97	56	51	84	84	25	97	53	19	51	75	67	94	78	47	61
15	90	10	96	55	49	83	82	23	96	53	18	50	76	68	96	80	50	64
16	89	09	95	53	47	81	81	22	95	52	18	50	76	69	98	82	52	67
17	89	08	93	52	45	80	79	21	94	52	17	49	76	70	1·00	84	55	70
18	89	06	92	50	43	78	78	19	93	51	17	49	77	71	02	86	58	73
19	88	05	91	49	41	77	76	18	92	50	16	48	77	72	04	88	61	76
20	88	04	89	47	39	75	75	16	90	49	15	47	77	72	05	89	63	79
21	88	03	88	46	37	74	73	15	89	48	14	46	77	73	07	91	65	81
22	87	01	86	44	35	72	72	14	87	47	14	46	78	74	08	92	68	84
23	87	00	85	43	32	71	70	12	86	46	13	45	78	75	10	94	70	87
24	87	9·99	83	41	30	69	69	11	85	45	12	44	78	76	11	95	72	90
25	86	98	82	40	28	67	67	10	83	44	11	43	78	77	13	97	75	92
26	86	97	80	38	26	66	65	·08	82	43	11	42	78	77	14	98	77	95
27	86	·96	79	37	24	61	64	07	81	42	10	41	78	77	16	99	80	98
28	85	95	77	35	21	63	62	05	79	41	09	41	78	77	17	28·01	82	35·01

1822.	1α Libræ	2α Libræ	α Cor. Bor.	α Serpentis	Antares.	α Herculis.	α Ophiuchi.	α Lyræ	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1α Capri.	2α Capri	α Cygni	α Aqua.	Fomalhaut	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
Feb.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	52·53	3·97	10·07	31·20	30·99	32·38	40·78	54·12	47·57	5·60	33·93	46·32	9·07	20·62	38·18	47·68	53·87	12·22
2	56	4·00	11	23	31·02	41	81	15	59	62	95	34	09	63	18	68	87	21
3	60	04	14	26	06	44	83	17	61	64	97	35	10	64	18	68	87	20
4	63	07	17	29	09	46	86	20	63	66	98	37	12	65	19	68	87	19
5	66	10	20	33	12	49	89	22	64	67	34·00	38	13	66	19	68	86	19
6	69	13	23	36	16	52	92	25	66	69	01	40	15	67	20	68	86	18
7	73	17	27	39	20	55	94	27	68	71	03	42	17	68	20	68	86	17
8	76	20	30	42	23	58	97	30	70	72	05	44	19	69	21	68	86	16
9	79	23	33	45	27	61	41·00	33	72	74	07	46	21	71	22	68	86	16
10	83	27	37	48	30	64	03	36	74	76	09	48	23	72	23	69	86	15
11	86	30	40	51	34	67	06	38	76	78	11	49	24	74	23	69	86	14
12	89	33	43	54	38	70	09	41	78	80	13	51	26	75	24	69	86	14
13	92	36	46	57	41	73	12	44	80	83	15	53	28	77	25	69	86	13
14	95	39	50	60	45	76	15	46	82	85	17	55	30	78	26	69	87	12
15	99	43	53	64	48	79	18	49	84	87	19	57	32	80	26	69	87	12
16	53·01	45	56	67	51	82	21	52	86	89	21	59	34	81	27	70	87	11
17	05	49	59	70	55	85	24	55	88	90	23	61	36	83	28	70	88	11
18	09	53	63	73	59	88	27	58	90	93	25	63	38	85	29	70	88	10
19	11	55	66	76	62	91	30	61	93	95	27	65	40	87	30	71	88	10
20	14	58	69	79	66	94	33	64	95	98	30	68	43	89	31	71	88	10
21	17	61	73	82	69	97	36	67	97	6·00	32	70	45	91	32	72	89	09
22	20	64	76	86	73	33·00	39	70	99	02	34	72	47	93	33	72	89	09
23	23	67	79	89	76	03	41	73	48·02	04	37	74	49	95	34	73	90	09
24	26	70	82	92	80	06	44	76	04	07	39	76	51	97	36	74	90	08
25	29	73	85	95	83	10	47	80	06	09	41	80	55	99	37	74	91	08
26	32	76	88	98	87	13	50	83	09	11	43	81	56	21·01	38	75	91	08
27	35	79	92	32·01	90	16	54	86	11	14	46	83	58	03	39	76	92	07
28	38	82	95	04	94	19	57	89	14	16	48	85	60	06	40	77	92	07

1822.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 52	H. M. 4 25	H. M. 5 3	H. M. 5 6	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 30	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 40	H. M. 11 41	H. M. 13 15	H. M. 14 7
Mar.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	4·85	9·93	59·76	44·33	35·19	0·61	4·60	34·04	19·77	16·40	1·07	27·40	52.77	55·77	1.18	28·02	51·84	35·04
2	85	92	75	32	16	59	58	02	76	38	06	39	77	78	19	03	86	06
3	85	92	74	30	14	58	57	01	74	37	05	37	77	78	20	04	88	08
4	85	91	72	28	12	56	55	33·99	72	36	04	36	77	78	21	06	90	11
5	85	90	71	27	10	54	53	98	71	34	03	35	76	78	23	07	92	13
6	85	89	70	25	07	53	51	96	69	33	01	33	76	78	24	08	94	15
7	85	88	69	24	05	51	50	95	68	32	00	32	76	78	25	09	96	18
8	86	87	68	22	03	50	48	93	66	31	0·99	31	75	78	26	10	98	20
9	86	86	67	21	00	48	47	92	65	30	98	29	75	78	27	11	52·00	22
10	86	85	65	19	34·98	46	45	90	63	28	97	28	75	78	28	12	02	25
11	87	84	64	17	96	44	43	88	61	26	96	26	74	78	29	13	04	27
12	87	84	63	16	93	42	42	87	59	25	64	25	74	77	30	14	06	28
13	88	83	62	14	91	41	40	85	58	23	63	24	73	77	30	14	07	31
14	88	82	61	13	89	39	38	84	56	22	62	22	72	77	31	15	09	33
15	89	82	60	11	87	37	36	82	55	20	60	21	72	76	32	16	11	35
16	89	81	59	10	84	36	34	81	53	18	89	18	71	76	32	16	12	38
17	89	81	58	09	82	34	33	79	51	17	88	17	70	76	33	17	14	40
18	90	80	57	07	80	32	31	77	50	15	86	16	70	76	34	18	15	42
19	90	80	56	06	78	31	29	76	48	14	85	15	69	75	34	18	17	44
20	91	79	56	04	75	29	27	74	46	12	84	13	68	75	35	19	19	46
21	* 92	79	55	03	73	27	26	72	44	10	82	12	67	75	35	19	20	48
22	94	79	54	01	71	26	24	71	42	08	81	10	66	74	35	19	22	50
23	95	78	53	00	69	24	23	69	40	06	79	08	65	74	36	20	23	52
24	96	78	52	43·99	67	23	21	68	38	05	78	07	64	73	36	20	24	53
25	96	78	52	98	65	21	19	66	37	03	76	05	63	73	36	20	25	55
26	97	77	51	96	63	20	17	65	35	01	75	04	62	72	37	21	27	57
27	98	77	50	95	60	18	16	63	33	15·99	73	02	61	71	37	21	28	59
28	99	77	49	94	59	17	14	62	31	97	72	00	60	70	37	21	29	60
29	99	76	48	92	56	15	12	60	30	96	70	26·99	59	70	38	21	30	62
39	5·00	76	48	91	54	13	11	59	28	94	69	97	58	69	38	22	32	64
31	01	76	47	90	52	12	09	57	26	92	67	95	57	68	38	22	33	65

1822	1α Libræ	2α Libræ	α Cor Bor	α Serpentis	Antares	α Herculi	α Ophiuchi	α Lyræ	γ Aquilæ	α Aquilæ	β Aquilæ	1α Capri	2α Capri	α Cygni	α Aquarii	Fomalhaut	α Pegasi	α Andromedæ
	H M 14 40	H M 14 41	H M 15 27	H M 15 35	H M 16 18	H M 17 6	H M 17 25	H M 18 30	H M 19 37	H M 19 42	H M 19 46	H M 20 7	H M 20 8	H M 20 35	H M 21 56	H M 22 47	H M 22 55	H M 23 59
Mar	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s
1	53 40	4 84	10 98	32 07	31 98	33 22	41 60	54 92	48 16	6 19	34 51	46 98	10 63	21 08	38 41	47 78	53 93	12 07
2	43	87	11 01	10	32 01	25	63	95	19	21	53	90	65	11	42	79	94	07
3	46	90	04	13	05	28	66	98	21	24	56	93	68	13	44	80	94	07
4	48	92	07	16	08	31	69	55 02	24	26	58	95	70	16	45	81	95	07
5	51	95	10	19	11	34	72	05	26	29	60	98	73	18	47	82	96	07
6	54	98	13	21	15	37	75	08	29	31	63	47 00	75	21	48	83	97	08
7	57	5 01	16	24	18	40	78	11	31	34	65	03	78	23	50	84	98	08
8	59	03	19	27	21	43	81	14	34	36	68	05	80	26	51	85	99	08
9	62	06	22	30	25	46	84	17	37	39	70	07	82	28	52	86	54 00	09
10	65	09	25	33	28	50	87	21	39	41	73	10	85	31	54	87	01	09
11	68	12	28	35	31	53	90	24	42	44	76	13	88	34	56	89	02	09
12	70	14	31	38	35	56	93	28	44	48	79	16	91	37	58	90	03	10
13	73	17	33	41	38	59	97	31	47	49	81	18	93	39	60	91	05	10
14	75	19	36	43	41	62	42 00	35	50	52	84	21	95	42	61	93	06	11
15	78	22	39	46	45	65	03	38	52	55	86	24	99	45	63	94	07	11
16	80	24	42	49	48	68	06	41	55	57	89	26	11 01	48	65	96	08	12
17	82	26	45	51	51	71	09	45	58	60	92	29	04	51	67	97	09	13
18	85	29	47	54	54	74	02	48	60	62	94	32	07	53	68	98	10	13
19	87	31	50	56	57	77	05	51	63	65	97	34	09	56	70	99	12	14
20	90	34	53	59	61	80	18	55	66	68	35 00	37	12	59	72	48 01	13	15
21	92	36	55	62	64	83	21	59	69	71	03	40	15	63	74	03	14	16
22	94	38	58	64	67	86	24	62	72	74	06	43	18	65	76	05	15	17
23	96	40	60	67	70	89	27	65	75	77	09	46	21	69	78	06	17	18
24	99	43	63	69	74	91	30	69	78	80	12	49	24	72	80	08	19	19
25	54 01	45	65	72	77	94	33	72	80	83	14	52	27	75	82	10	20	20
26	03	47	68	74	80	97	36	76	83	85	17	55	30	78	84	12	22	21
27	05	49	70	77	83	34 00	39	79	86	88	20	57	32	82	86	14	23	22
28	07	51	73	79	86	03	41	82	89	91	23	60	35	85	88	15	25	23
29	09	53	75	82	89	06	44	86	92	94	26	63	38	88	90	17	26	24
30	12	56	78	84	92	09	47	90	95	97	29	66	41	91	92	19	28	25
31	14	58	80	86	95	12	50	93	98	7 00	31	69	44	95	95	21	30	26

1822	γ Pegasi	α Arietis	α Ceti	Aldebaran	Capella	Rigel	β Tauri	α Orionis	Sirius	Castor	Procyon	Pollux	α Hydræ	Regulus	β Leonis	β Virginis	Spica Virginis	Arcturus
	H. M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M	H M
	0 4	1 57	2 52	4 25	5 3	5 6	5 15	5 45	6 37	7 23	7 30	7 34	9 18	9 58	11 40	11 41	13 15	14 7
April	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s
1	5 02	9 76	59 47	43 89	34 50	0 11	4 08	33 56	19 24	15 90	0 66	26 94	52 56	55 67	1 38	28 22	52 34	35 67
2	03	76	46	87	48	09	06	55	22	89	64	92	55	67	38	22	35	68
3	05	76	46	86	47	08	05	53	21	87	63	90	54	66	38	22	36	69
4	06	76	45	85	45	07	03	52	19	85	61	89	53	65	38	22	37	71
5	07	77	45	84	43	05	02	50	17	84	60	87	52	64	38	22	38	72
6	08	77	44	83	41	04	01	49	16	82	58	85	51	63	38	22	39	73
7	09	77	44	82	39	02	3 99	47	14	80	57	84	50	62	38	22	40	75
8	11	77	43	81	37	01	98	46	12	78	55	82	48	61	38	22	41	76
9	12	78	43	80	36	00	96	44	10	77	54	80	47	60	38	22	42	78
10	14	78	43	79	34	5 59 98	95	43	08	75	52	78	46	59	37	22	43	79
11	16	79	43	78	33	97	94	41	07	73	50	76	44	58	37	21	44	80
12	18	79	43	78	31	96	92	40	05	71	49	75	43	57	37	21	44	81
13	20	80	43	77	30	95	91	39	03	70	47	73	42	56	36	21	45	82
14	22	80	43	76	28	94	90	38	02	68	46	71	41	55	36	21	46	83
15	23	81	43	75	27	93	89	37	00	66	45	70	39	54	36	20	46	84
16	24	81	43	74	25	92	88	36	18 99	64	43	68	38	53	35	20	47	85
17	26	82	43	74	24	90	87	35	97	63	42	66	37	52	35	20	48	86
18	27	82	43	73	22	89	85	33	96	61	40	65	36	50	34	19	48	87
19	29	83	43	72	21	88	84	32	94	59	39	63	34	49	34	19	49	88
20	31	84	43	72	20	87	83	31	93	58	37	62	33	48	33	18	50	89
21	33	* 85	44	71	19	86	82	30	91	56	36	60	32	47	33	18	50	90
22	35	87	44	71	18	86	82	29	90	55	35	59	31	46	32	17	51	90
23	37	88	44	71	17	85	81	28	89	53	33	57	30	45	32	17	51	91
24	39	89	45	70	16	84	80	27	87	51	32	56	29	44	31	16	52	92
25	41	90	45	70	15	83	79	26	86	50	30	54	27	42	31	16	52	92
26	43	91	45	70	14	83	78	25	84	48	29	53	26	41	30	15	52	93
27	45	93	46	70	13	82	78	24	83	47	28	51	24	40	30	15	53	94
28	47	94	46	70	12	81	77	23	82	45	26	50	23	39	29	14	53	94
29	50	95	47	69	11	80	76	22	80	44	25	48	21	38	29	14	53	95
30	52	97	48	69	11	80	76	21	79	42	24	47	19	37	28	13	53	95

1822	1 α Libræ.	2 α Libræ.	α Cor. Bor.	α Serpentis.	Antares.	α Herculis.	α Ophiuchi.	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1 α Capri.	2 α Capri.	α Cygni.	α Aqua.	Fomalhaut.	α Pegasi.	α Andromedæ.
	H M 14 40	H M 14 41	H M 15 27	H M 15 35	H M 16 18	H M 17 6	H M 17 26	H M 18 30	H M 19 37	H M 19 12	H M 19 46	H M 20 7	H M 20 8	H M 20 35	H M 21 56	H M 22 47	H M 22 55	H M 23 59
April	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s	s
1	54 15	5 59	11 82	32 88	32 98	34 15	42 53	55 97	49 01	7 03	35 34	47 72	11 47	21 98	38 97	48 23	54 32	12 27
2	17	61	84	91	33 01	17	56	56 00	04	06	37	75	50	22 02	99	26	34	29
3	19	63	86	93	04	20	59	04	07	09	40	77	53	05	39 02	28	36	30
4	21	65	88	95	07	23	61	07	10	12	43	81	56	09	04	30	38	32
5	23	67	90	97	09	25	64	10	13	15	46	84	59	12	07	32	40	33
6	25	69	92	99	03	28	67	14	16	18	49	87	62	16	09	34	42	35
7	26	70	95	33 02	05	31	70	17	19	21	52	90	65	19	11	37	44	36
8	28	72	97	04	08	34	73	20	22	24	55	93	68	22	14	39	46	38
9	30	74	99	06	21	37	76	24	25	27	58	96	72	26	16	41	48	39
10	32	76	12 01	08	23	40	79	28	28	30	61	48 00	75	30	19	44	50	41
11	34	77	03	10	26	42	81	31	31	33	64	03	78	34	22	46	53	42
12	35	79	05	12	29	45	84	34	34	36	67	06	81	37	24	48	55	45
13	36	80	07	14	31	47	87	37	37	39	70	09	84	41	27	51	57	47
14	38	82	09	16	34	50	89	41	40	41	73	12	87	45	29	53	59	49
15	40	84	10	17	37	52	92	44	43	44	76	15	90	48	32	56	61	50
16	41	86	12	19	39	55	95	47	46	47	79	18	93	52	34	58	63	52
17	43	87	14	21	42	57	97	50	49	50	81	21	96	55	37	61	66	54
18	44	88	16	23	44	60	43 00	54	52	53	84	24	99	59	39	64	68	56
19	46	90	18	25	47	62	03	57	56	56	89	28	12 03	63	42	66	70	58
20	47	91	19	27	49	65	05	60	59	61	92	31	06	67	45	69	72	60
21	49	93	21	28	52	67	08	64	62	64	95	34	09	70	48	72	75	62
22	50	94	22	30	54	69	10	67	65	67	98	37	12	74	51	75	77	64
23	51	95	24	31	56	72	13	70	68	70	36 01	40	15	78	53	78	80	67
24	52	96	25	33	59	74	15	73	71	73	04	44	19	82	56	80	82	69
25	53	97	27	35	61	76	18	76	74	76	07	47	22	85	59	83	85	71
26	55	99	28	36	63	79	20	79	77	79	10	50	25	89	62	86	87	73
27	56	00	29	38	65	81	23	82	80	82	13	53	28	92	64	88	90	75
28	57	6 01	31	39	68	81	25	85	83	85	16	56	31	96	67	91	92	77
29	59	03	33	41	70	86	28	89	86	88	19	60	35	23 00	70	94	95	80
30	58	02	34	43	73	88	30	91	89	91	22	63	38	04	73	97	97	83

X. *Trial of the Meridian Circle, made by* Reichenbach *for the Observatory at Königsberg. By M.* Bessel *

I hoped to be enabled to furnish you with a complete account of the observations hitherto made with the meridian circle of Reichenbach; but several examinations (which appear to me to be necessary) have not yet been completed; partly on account of the very bad weather in this year, and partly on account of the late arrival of a particular microscopical apparatus which Privy Counsellor Pistor has not only admirably contrived, but also correctly executed for me. This apparatus has been in my possession about a month, and I have already attained by it the object I had in view: but, there are still some things which I must ascertain before I can assert that my declinations are so correctly determined as this beautiful instrument seems capable of doing. I could, indeed, produce many observations, such as the instrument has given them; and I could add that several improvements (which are not yet made) will be very trifling, and that I might even effect them by approximation: but yet all my data would be only preliminary, and these appear to me to be little interesting, since we already possess several similar data which want, more or less, the required confidence. I shall therefore, for the present, pass over the declinations in total silence.

On the other hand, I have completed a very severe trial of the instrument in regard to the Right Ascensions; which I shall give in the 6th part of my Observations, now in the press: and I shall there prove that, from the nature of the instrument and its correcting property, no constant error can arise. This part of the observations I consider therefore to be already completed, with the exception of what may properly be called the accidental errors of observation; which, in comparison with the mean constant errors, are of no importance, and which moreover appear clearly enough from the observations themselves. I think therefore that I may venture to give you some results.

Mr. Pond has given, in the Nautical Almanac for 1823, a new catalogue of the right ascensions of the principal stars for 1820, founded on some solar observations of his own. These being reduced to the year 1815 (by comparing their proper motions with the catalogue for 1755) we have the following differences between his values and mine. I should, however, previously remark that I have altered the double star *α Geminorum* 0″,20; Mr. Pond having observed the second of the two stars, whilst I observe the mean of the two: and moreover that there is in the above-mentioned Nautical Almanac an obvious error of 1″ in the place of *α Scorpii*.

* From Bode's *Astronomische Jahrbuch* for 1824, page 232.

γ Pegasi

γ Pegasi	..	+0″,021	α¹ Libræ	..	+0″,032
α Arietis	..	+0,036	α² ——	..	+0,055
α Ceti	..	−0,042	α Coronæ	..	+0,081
α Tauri	..	+0,131	α Serpentis	..	+0,074
α Aurigæ	..	+0,137	α Scorpii	..	−0,105
β Orionis	..	+0,119	α Herculis	..	+0,128
β Tauri	..	+0,161	α Ophiuchi	..	+0,183
α Orionis	..	+0,213	α Lyræ	..	+0,137
α Canis Maj.		+0,113	γ Aquilæ	..	+0,104
α Gemin.	..	+0,206	α ——	..	+0,088
α Canis Min.		+0,196	β ——	..	+0,062
β Gemin.	..	+0,146	α¹ Capricorni		+0,014
α Hydræ	..	+0,257	α² ——	..	−0,042
α Leonis	..	+0,214	α Cygni	..	+0,116
β ——	..	+0,158	α Aquarii	..	+0,028
β Virginis	..	+0,123	α Piscis Aust.		−0,142
α ——	..	+0,073	α Pegasi	..	+0,047
α Bootis	..	+0,137	α Andromedæ		+0,090

The *mean* of these differences is +0″,093; being about the quantity by which Mr. Pond's right ascensions, in the whole, exceed mine. You will remember that the observations, made with my former instruments, induced me to add +0″,241 to Dr. Maskelyne's determination of α *Aquilæ*: Mr. Pond has now added still more. But, neither of these determinations has yet that agreement which might be desired in so momentous an object, the foundation of all astronomical observations. The probable error of Mr. Pond's determinations is not pointed out; that of mine is 0″,0235. It is therefore yet doubtful, whether there is here a constant error, or an accidental one which would disappear by continued observations. In the mean time I have been desirous to know, what result the meridian circle of Reichenbach would give respecting it; and I have therefore calculated 25 observations of the sun from 27th March to 16th September 1820. According to these observations the amendment of my former catalogue is +0″,006 in time; which is absolutely imperceptible. It convinces me, however, that I should not be justified in deciding on the difference between Mr. Pond and myself; since the observations of *one* year (although they should be free of all constant errors of the instrument, as I have reason to believe is the case with mine) are not yet sufficient to decide so nice and difficult a point. According to my ideas, a long continuation of observations is requisite, if we wish to have the most accurate results. I have indeed sometimes found that a well according series of observations will deviate further from another series than the probable errors would lead us to believe.

From

From the change of the daily and annual temperature, the degree of light, &c. &c. as well as from the reductions which must be applied in observations, small errors may arise, which perhaps we shall never learn how to bring into account; but which, by a continuation through several seasons, we may render of less importance.

If we deduct the *mean difference* of the two catalogues from the several differences above given, the result of Mr. Pond's individual determinations in reference to mine will more clearly appear; and as this comparison gives occasion for some observations, I will here insert it.

γ Pegasi	.	−0,072	α^1 Libræ	..	−0,061
α Arietis	.	−0,057	α^2 ——— ..	..	−0,038
α Ceti	..	−0,135	α Coronæ	..	−0,012
α Tauri	..	+0,038	α Serpentis	..	−0,019
α Aurigæ	..	+0,043	α Scorpii	..	−0,198
β Orionis	..	+0,026	α Herculis	..	+0,035
β Tauri	.	+0,068	α Ophiuchi	..	+0,090
α Orionis	..	+0,120	α Lyræ	..	+0,044
α Canis Maj.		+0,020	γ Aquilæ	..	+0,011
α Gemin.	..	+0,113	α ——— ..	..	−0,005
α Canis Min.		+0,103	β ——— ..	..	−0,031
β Gemin.	..	+0,053	α^1 Capricorni		−0,079
α Hydræ	..	+0,164	α^2 ———	..	−0,135
α Leonis	..	+0,121	α Cygni	..	+0,023
β ——— ..	..	+0,065	α Aquarii	..	−0,065
β Virginis	..	+0,030	α Piscis Aust.		−0,235
		−0,020	α Pegasi	..	−0,046
α Bootis	.	+0,044	α Androm.	..	−0,003

It is certainly difficult to form an agreement in the hundredths of the second of time; and, with half the stars, the differences are below 0″,05. But it is yet very improbable that these differences should all have arisen from accidental errors of observations. We may even perceive a regularity in their march; for, in the vicinity of *α Canis minoris*, the positive quantities clearly preponderate; whilst in the southern stars, the negative are most numerous. The first of these discrepancies would be explained if, in my catalogue, *α Canis minoris* (which star is a point of comparison for the others) were incorrectly determined: and, in fact, if it were one-tenth of a second too small. This however is improbable; since its determination is founded on 75 observations. Yet it cannot be denied that the stars, situated at a distance of 12 hours from each other, offer the greatest difficulties; partly from the going of the clock, and partly from the

the influence of temperature on the adjustments of the instruments. In my "Treatise on the Fundamental Catalogue," I have given data which confirm the correctness of my former determinations; yet the opportunity for a *new* trial, which the particular excellence of the new erection (*aufstellung*) and the admirable regularity of *Repsold's* clock offered to me, were particularly favourable for this purpose. I therefore reduced the 25 observations that have as yet occurred, and find for 1820 as follows:

1820.			H. M. S.	1821.			H. M. S.
March	22	=	7.29 52,70	February	8	=	7.29.52,49
June	23	..	37		9	..	69
July	28	..	37		11	..	54
	30	..	44		13	..	57
August	5	...	52		27	..	32
	8	..	44	March	23	..	45
	29	..	62		24	..	64
September	8	..	47		25	..	63
	9	..	40		26	..	50
	11	..	44		29	..	56
	13	..	37		30	..	59
	15	..	32		31	..	33
December	30	..	58				

The mean is $7^h\ 29'\ 52'',494$, or only $0'',033$ greater than my determination for 1815. So that the correctness of the former determination is hereby confirmed. I also believe that the method employed by Mr. Pond will not protect him from a *constant error* in opposite groups of stars. By his method a star is reduced with the mean of all that have been observed on the same day, according to the data in Dr. Maskelyne's catalogue; and the new catalogue thence arising is settled with reference to the equinoxes. This method would be strictly correct, if the 36 stars were all observed in one day; but as there will be many more days, where the stars in one quarter of the heavens are observed alone, than where they are observed at the same time with those opposite thereto, it is evident that an error arising in such quarter can only disappear in part; and the less so the more frequently the stars are observed in such particular quarter: since thereby the error obtains a greater preponderance. As Mr. Pond's determinations are founded on 151 observations with a celebrated instrument, and by so celebrated an astronomer, and as my determinations too have received undeniable confirmation; I can see only this mode of accounting for the discordancies.

The

The other difference of the two catalogues (viz. that the southern stars have, according to Mr. Pond, much less right ascension than with me) seems to proceed from a *constant error in the fixing of one of the two transit instruments.* This difference shows itself very clearly in the comparison above mentioned; and amounts, in the case of *α Piscis Australis*, to as much as 0″,235. It may indeed have its origin in a bend of the telescope, or in a wrong determination of the line of collimation (perhaps produced by the former), since this must occasion an erroneous reduction to the meridian. Assuming that the transit instrument describes a great circle, then an error of the collimation, $=\Delta c$, has the influence Δc tang $(45 - \frac{1}{2}\delta)$, on the reduction; provided the correction is determined by the pole star. This error of the collimation however would be so great, that it could not escape the observer; whence it is not improbable, that there are still other causes which have occasioned a deviation from the meridian.

The above-mentioned rigorous trial, of the meridional circle of Reichenbach, was principally directed to this point: and I believe I shall be able to prove, that the method, followed by me, cannot leave a perceptible doubt in the determination of the collimation. I have thence been enabled also in this respect to try by new observations my former data, and shall mention here what I obtained for *α Scorpii*, and *α Piscis Australis*, viz. for 1820

α Scorpii	$=16^h.18^m.23^s,219$	25 obs.
α Piscis Aust.	$=22.\ 47.\ 41\ ,197$	21 obs.

differing from my former determination −0″,088 and +0″,0771; and from that of Mr. Pond +0″,112 and +0″,330. So that, in this too, the new observations speak in favour of the Königsberg Catalogue.

In a few years I hope to be able to give a perfectly new fundamental Catalogue. I merely undertook the present preliminary investigation of some stars, in order to ascertain whether any *constant errors* have crept into my former catalogue, in spite of every precaution. I believe that I may apprehend this less now than before.

XI. *On Addition and Subtraction of Algebra.* *By Mr.* PAUL NEWTON.

To Dr. Tilloch.

Old Assembly House, Newark, Jan. 3, 1822.

SIR, — ALL those authors who have treated on Addition of Algebra, at least all those authors (a numerous class) to whose works

works I have had access, make no essential difference between some parts of Addition and some parts of Subtraction of Algebra. From an attentive consideration of the subject, I feel persuaded that the operations of Addition should be restricted to quantities, whether like or unlike, which have like signs. That part of Addition which is employed in collecting quantities, whether like or unlike, which have unlike signs, should be classed under the rule for the Subtraction of *simple quantities*. Our authors define clearly enough that the sign $+$ denotes Addition, and that the sign $-$ denotes Subtraction; they then blend these signs, or blend the quantities to which these signs are prefixed, and sometimes call the mixture Addition, and sometimes call it Subtraction. Dissatisfied with this procedure, Mr. Bonnycastle recommends *new names* for these two primary rules; as if there were some secret charm in a name absurdly applied. If any objection lie against the term Subtraction, as Mr. Bonnycastle supposes and affirms, that objection may be obviated by removing the cause, whether real or imaginary. "The incongruous mixture," as Mr. Bonnycasle styles it, may be removed or avoided, if offensive, by transposing the negative term or terms from the minuend to the subtrahend, and by transposing, also, the negative term or terms from the subtrahend to the minuend; by which means, we shall have nothing but positive terms in the minuend, and nothing but negative terms in the subtrahend. Thus, retaining the old form of writing the quantities, if from

$$4x + a - b$$

we take $4x + b - 3a$,

we shall obtain, by transposing the negative terms, this arrangement, viz,

$$4x + a + 3a$$
$$-(4x + b + b)$$

or this, viz. $4x + 4a - 4x - 2b = 4a - 2b =$ diff. required.

For, the difference between any two quantities will remain the same, whether we equally augment or equally diminish them. Thus, let a exceed b; and, to avoid ambiguity, let a, as well as b, exceed c; then will $a-b=(a+c)-(b+c)=(a-c)-(b-c)$. Therefore, if from $4x + a - b$

we take $4x + b - 3a$,

first, augment both quantities by b, and we obtain this arrangement, viz. From $4x + a$

Take $4x + 2b - 3a$.

Now augment both quantities by $3a$, and we shall have this arrangement, viz. From $4x + 4a$

Take $4x + 2b$,

or this, viz. $4x + 4a - (4x + 2b) = 4a - 2b =$ diff. as before.

Both quantities, in a manner analogous to the method employed

ployed in common arithmetic, have been equally augmented, and the augment of each quantity, in this case, is $= 3a + b$; but the difference between these augmented quantities is, as I have shown, the same as it would have been between the original proposed quantities. I am sir, very respectfully,

Your obedient humble servant,

PAUL NEWTON.

XII. *A Question addressed to the Rev.* J. GROOBY, *respecting the Tables employed by him in calculating the Corrections of Dr.* MASKELYNE'S 36 *Stars. By* A CORRESPONDENT.

To Dr. Tilloch.

SIR,—WILL you permit me, through your Journal, to ask your correspondent Mr. Grooby, *what* tables of Professor Bessel's he makes use of in the calculation of the Corrections of Maskelyne's Stars. I have not heard of any except those annexed to his Observations, and I do not find that they give the same corrections as Mr. G. uses, though very near to it. It is a circumstance not generally known, perhaps, to your astronomical readers, that the Professor himself does not use his *own* tables in reducing his observations; as any one may satisfy himself, if he will only take the trouble of reducing a few of the transits he has published, and comparing the results with the corrected Right Ascension given by the Professor himself. I have calculated some hundreds, and never found one agree: hence I had supposed that I must have mistaken his *mode* of calculation, particularly as he has given no example of his method of using his tables. But in his *Astronomiæ Fundamenta* he *has* given examples, and, what is most extraordinary, not one of the corrections in those examples agrees with the one given by the table. Give me leave to notice one more particularly,—and I will take the first. Where the correction of *α Lyræ* is required on the 13th of December 1756. Adding, as the two preliminary tables direct, 1.56, I am to look out in Table 1st for the correction answering to December 14. 56. Now opposite to December 6. I find +0.273, and opposite to December 16. +0.246, difference ,027. I say therefore,—As 10 days is to ,027, so is 8.56 to ,023; which, as the numbers are diminishing, is to be subtracted from +0.273, and give +0.250 for the correction; but in the example it is +0.247. This, it is true, is no great difference; but small as it is, it ought not to be, and Mr. Grooby will perhaps find some difficulty in accounting for it, as well as in maintaining his opinion, that *M. Bessel's Tables are the most correct yet published.*

I am sir, your obedient servant,

OBSERVER.

XIII. *On*

XIII. *On the Temperature of a Room indicated by two Thermometers at different Altitudes.* By JOHN MURRAY, *F.L.S. M.W.S. &c. &c.*

To Dr. Tilloch.

SIR, — ON my return from the Continent in 1819, I brought with me Breguet's "*Thermomètre Métallique,*"—an instrument susceptible of the most delicate sensibility, and with which I have made many interesting experiments. In a still room without a fire, in the summer months, it readily communicated the difference in temperature between the floor and a chair, and this last and the table.

The phænomena induced me to make a series of experiments on the difference of temperature indicated by two thermometers at different altitudes, yet otherwise under similar circumstances.

In the first series of experiments made at Nottingham from the 21st to the 28th October last inclusive, I was surprised to find, that any deviation from its uniformity had an immediate relation to the radiation of the terrestrial temperature to the heavens;—indeed, I am much deceived if the difference in question may not be found as accurate a guide as the barometer itself. Since I came to London, I have kept a pretty accurate register of the difference between two thermometers: one placed with its bulb on the floor, and the other suspended 6½ feet above it, embracing the period betweeen 5th and 24th November. On the 11th I began first to note the phænomena of the weather in correspondence with these changes, and it will be there seen, that when the difference exceeds 2° to 2·5 F. the weather has been variable and wet. The following comprise the results of the observations in a tabular form.

The following experiments were made at Nottingham with thin calico curtains to the windows of my room.

Date of Obser.	Hour.	Pos. of Therm.	Temp. indicated.	Dif. between	
1821.	H. M.				
Oct 21	9 30 A. M	Floor	56°5 F.	1° F.	
		6½ feet	57·5		
	9 30 P. M.	Floor	58	1·5	
		6½ feet	59·5		
24	7 30 P. M.	Floor	59	2	
		6½ feet	61		
25	2 P. M.	Floor	59	2	
		6½ feet	61		
	8 P. M.	Floor	60·5	3 5	
		6½ feet	64		
26	10 30 P. M.	Floor	63	2 5	
		6½ feet	66		
28	9 P. M.	Floor	70·5	2	
		6½ feet	72·7		

Note of experiments made in London, with two Thermometers at different altitudes.—Shutters of wood to the room.

Date of Obser.	Period of Day.	Pos. of Therm.	Temp. indicated.	Dif. between.	Weather.
1821.	H. M.				
Nov. 5	9 30 A. M.	Floor	53°·5 F.	1°·5 F.	
		6½ feet	55		
	7 P. M.	Floor	55·5	3·5	
		6½ feet	59		
6	9 15 A. M.	Floor	53	2·25	
		6½ feet	55·25		
	9 30 P. M.	Floor	54·5	2·5	
		6½ feet	57		
7	9 A. M.	Floor	54·5	2·5	
		6½ feet	57		
	5 P. M.	Floor	54	2·5	
		6½ feet	56·5		
8	9 30 A. M.	Floor	55·5	2·5	
		6½ feet	58		
	6 15 P. M.	Floor	55·5	3·5	
		6½ feet	59		
9	9 30 A. M.	Floor	54	2·5	
		6½ feet	56·5		
	5 30 P. M.	Floor	55	2	
		6½ feet	57		
10	10 30 A. M.	Floor	56	2·5	
		6½ feet	58·5		
	7 30 P. M.	Floor	59·5	3	
		6½ feet	62·5		
11	9 30 A. M.	Floor	59·5	1	
		6½ feet	60·5		
	8 P. M.	Floor	63·5	5·5	Continued rain
		6½ feet	69		
12	9 30 A. M.	Floor	59·5	2·5	Fine
		6½ feet	62		
	5 30 P. M.	Floor	59	2·5	Clear evening
		6½ feet	61·5		
13	9 15 A. M.	Floor	56	2	Foggy
		6½ feet	58		
	9 30 P. M.	Floor	61	4	Slight rain, and during the night incessant.
		6½ feet	65·5		
14	9 30 A. M.	Floor	62·5	2·5	Cloudy
		6½ feet	65		
	6 P. M.	Floor	64	3·5	Rain
		6½ feet	67·5		
	10 P. M.	Floor	64	4	Rain
		6½ feet	68		
15	10 A. M.	Floor	64	4	Rain
		6½ feet	68		
	6 30 P. M.	Floor	64	4	Rain
		6½ feet	68		
16	9 30 A. M.	Floor	61·25	3·5	Cloudy and rain
		6½ feet	64·75		
	[illegible] P. M.	Floor	63	4	Rain
		6½ feet	67		

Date of Obser.	Period of Day.	Pos. of Therm.	Temp. indicated.	Dif. between.	Weather.
1821.	H. M.				
Nov. 16	9 P. M.	Floor 6½ feet	64° F. 66·5	2°·5 F.	Clear star-light sky
17	9 30 A. M.	Floor 6½ feet	60 62·5	2·5	Bright unclouded sky
	6 30 P. M.	Floor 6½ feet	62 65·5	3·5	Some rain
	10 20 P. M.	Floor 6½ feet	63 68	5	Constant heavy rain
18	10 20 A. M.	Floor 6½ feet	61·5 64	2·5	Fine day
19	10 A. M.	Floor 6½ feet	59 63	4	Rain
	6 P. M.	Floor 6½ feet	58 62	4	Rain
	9 P. M.	Floor 6½ feet	61 63·5	2·5	Clear sky
20	10 A. M.	Floor 6½ feet	58 60	2·5	Fine day
21	10 A. M.	Floor 6½ feet	60 62·5	2·5	Fine day
	8 P. M.	Floor 6½ feet	58 63·5	5·5	Continued rain during the night
22	9 30 A. M.	Floor 6½ feet	57 59·5	2·5	Cloudy, but dry
	6 10 P. M.	Floor 6½ feet	60 64	4	Rain during the night
23	9 20 A. M.	Floor 6½ feet	59·5 62	2·5	Cloudy, but dry
	6 P. M	Floor 6½ feet	59·5 62	2 5	Fine evening
24	9 P. M.	Floor 6½ feet	57 59·5	2 5	Good day

I have only to regret occasional omissions, and that my avocations did not permit me to attend to more regular intervals of time.—The question appears to me to be a curious one, and to solicit further and more delicate attention. The correspondence is remarkable, though it will doubtless be violated by circumstances, which in the present state of our meteorological science cannot perhaps be always or altogether estimated.

I have the honour to be, sir,

Your most obedient and very humble servant,

J. MURRAY.

Surry Institution, January 11, 1822.

XIV. *No-*

XIV. *Notices respecting New Books.*

Recent Publications.

Architectural Antiquities of Rome, in 130 Engravings of Views, Plans, Elevations, Sections, and Details of the Ancient Edifices, in that City, with Historical, Descriptive, and Critical Accounts of the Style, Character, Construction and Peculiarities of each. By G. L. Taylor and Edward Cresy, Architects: to consist of 12 Numbers, imperial folio, 1*l.* 11*s.* 6*d.* each.—India paper, 2*l.* 2*s.*

Star Tables for the year 1822, for more readily ascertaining the Latitude and Longitude at Sea, during the Night. By Thomas Lynn, royal 8vo. 10*s.*

Solar Tables, being the Logarithmic versed Sines of Time, reduced to Degrees, commonly called Log rising, calculated to every Second of Time, and thereby facilitating the Operation of finding the Latitude by double Altitudes of the Sun or Stars, and the Longitude by Chronometer. By the same Author. 10*s.*

Evening Amusements; or, The Beauty of the Heavens displayed; in which several striking Appearances in the Heavens during the year 1822 are described. By W. Frend, 12mo. 3*s.* 6*d.* Bds.

A Natural Arrangement of British Plants, according to their relations to each other, as pointed out by Jussieu and others, including those cultivated for Use, with their Characters, &c. With an Introduction to Botany. By Samuel Frederick Gray, with 21 Plates. 2 vols. 8vo. 2*l.* 2*s.* Bds.

A Letter to Charles Henry Parry, M.D. &c. on the Influence of Artificial Eruptions in certain Diseases incidental to the Human Body. By Edward Jenner, M.D. &c. 4to. 5*s.*

Essays on Surgery and Midwifery, with Practical Observations and Select Cases, with Plates. By James Barlow, Surgeon. 8vo. 12*s.*

Treatise on Bulbous Roots, with Directions for their Cultivation. By the Hon. and Rev. William Herbert. 8vo. 5*s.*

The Botanical Register. By Sydenham Edwards, F.L.S. containing 8 coloured Specimens of exotic Plants. Number 82, price 4*s.*

Geraniaceæ; or Natural Order of Geraniums. By R. Sweet, F.L.S. Number 24, price 3*s.*—Continued Monthly.

The Botanical Cultivator; or A Practical Treatise on propagating, rearing, and preserving all Descriptions of Plants. By R. Sweet, F.L.S. 10*s.* 6*d.*

Rosarum Monographia; or A Botanical History of Roses, with an Appendix for the Use of Cultivators. By John Lindley, Esq. F.L.S. Royal 8vo. 21*s.*

The

The Eighth Number, completing the Views of the Cathedral Churches of England and Wales. By John Chessell Buckler.

The History and Antiquities of the See and Cathedral Church of Lichfield; illustrated by a Series of Engravings of Views, Elevations, Plans, and Details of the Architecture of the Church; with biographical Anecdotes of the Bishops of Lichfield and Coventry. By John Britton, F.S.A. 4to. pp. 50. 16 Engravings. 1*l.* 18*s.* Medium. 3*l.* 3*s.* Imperial.

Preparing for Publication.

MM. Spix and Martius, who have lately returned from a Voyage to the Brazi's, are preparing a detailed Account of their Observations, which will be published at the Expense of the King of Bavaria, with Charts, Plans, &c. The Plants which these Naturalists have collected in Brazil and sent to Munich, form already a Section of the grand Botanical Garden. The King has been pleased to confer on both of them the decoration of the Order of the Bavarian Crown.

M. Gamba, banker of Paris, has terminated his journeys through the provinces of Caucasus and Georgia, undertaken by order of the French Government in 1820 and 21. The numerous documents and articles which he has collected, are valuable in their relation to science, as well as to commercial and manufacturing interests. He was constantly attended in his travels by his son, M. J. Gamba, lieutenant of dragoons, who has just arrived in Paris from St. Petersburgh.

An Atlas of Ancient Geography, by S. Butler, DD. Author of Modern and Ancient Geography; also an Atlas of Modern Geography, by the same, are in considerable forwardness.

The Duke of Rutland has in the Press, A Tour through Belgium, embellished with Plates after Drawings by the Duchess.

In the Press, Cases illustrative of the Treatment of Diseases of the Ear, with practical Remarks relative to the Deaf and Dumb. By John Harrison Curtis, Aurist to the King, &c.

Instructions for Civil and Military Surveyors in Topographical Plan Drawing; forming a Guide to the just Conception and accurate Representation of the Surface of the Earth, in Maps and Plans. Founded upon the System of Major John George Lehmann. By William Siborn, Lieut. H.P. 9th Infantry. The Plates will be engraved by Lowry.

XV. *Pro-*

XV. *Proceedings of Learned Societies.*

ASTRONOMICAL SOCIETY OF LONDON.

Jan. 11.—A PAPER was read "On the Theory of Astronomical Instruments" by B. Gompertz, Esq. wherein the author, after stating the respective provinces of the practice and theory in relation to the construction of astronomical instruments, proceeds to divide them into two classes: viz. those constructed according to the best rules of the art, which he proposes to call *instruments formed by direct construction;* whilst others of inferior merit, and whose formation is not so perfect, he proposes to call *instruments formed by inverse construction.* The object of the paper is to examine the results which may be produced by instruments of the *latter* kind; and to show that, provided they are strong, and the parts, not intended for motion well fixed, a proper application of theory and observation will nevertheless enable the astronomer to obtain accurate results. His method is illustrated by several formulæ and examples.

A notice was also communicated from Mr. Bowdich, respecting some errors which appear to have crept into Mr. Park's calculations of the latitudes of several places in Africa. These errors seem to have arisen from Mr. Park having inadvertently reckoned on the month of April as having thirty *one* days; in consequence of which all his subsequent dates were incorrect. And when the declinations of the sun and moon were taken from the Nautical Almanac, for the purpose of computation, they were taken out for the wrong day. Mr. Bowdich gives a table of the corrected latitudes of upwards of twenty places; the differences of which vary from 1′ to 55′ from Mr. Park's calculations.

A paper was also read "on the collimation-adjustment of a transit instrument by circumpolar stars," by J. South, Esq. in which the author, after some remarks on the several modes of adjusting the collimation of a transit instrument, proposes the observation of certain circumpolar stars, whose slow motion renders them applicable to this purpose. He directs the instrument to one of these stars, when nearly on the meridian, and notes its transit over the first, second, and third wires: then, reversing the instrument, he notes its transit over the fourth and fifth wires; which are in fact the first and second wires already alluded to; and consequently the error of collimation (if any) is detected by a comparison of the intervals of time. The author then points out several advantages attending this plan; and suggests the propriety of adding a few more of such circumpolar stars to our fundamental catalogue, in order that their use, in this respect, may become more general.

XVI. *In-*

XVI. *Intelligence and Miscellaneous Articles.*

IODINE.

M. Heusmans read before the Society of Medicine of Louvain, at its sitting of the 16th of January, 1821, a paper upon the preparation of the tincture of iodine, and the re-establishment of that tincture deteriorated by time; as also on the non-existence of iodine in burnt sponge and in the ashes of the turf of Holland and the Netherlands. MM. Fyfe and Straub had announced the presence of this comburant in the ashes of Swiss turf.

As to the means of re-establishing the tincture of iodine, when by the decomposition of the alcohol it has passed into the acid state, it consists in infusing the tincture with an excess of super-oxide of manganese; the hydrogen is expelled, and the iodine regenerated; but the alcohol does not recover its primitive force, and is thus prevented from holding the iodine in solution. This tincture, which M. Heusmans exhibited to the Society, was of a deep brownish red, it stained the hands intensely, and contained 48 grains of iodine per ounce of alcohol, at 35° B.

This tincture is employed with success in the treatment of goitres and similar tumours.—*Annales Generales des Sciences Physiques.*

POLYHALITES.

The Polyhalite is a new mineral species established by Professor Stromeyer. It is in shapeless masses of a compact or fibro-lamellar texture; its fracture is irregular, it is middling hard, not scratching glass; its specific gravity is 2,7689; its colour is a brick-red, with the gloss of wax; it is translucid at the edges; it attracts humidity; it is *almost* soluble in boiling water; its solution is bitter and salt; it is easily fusible into an opaque mass of a whitish red. The analysis yields

Anhydrous sulphate of lime	22,4216
Sulphate of lime combined with water ..	28,2548
Anhydrous sulphate of magnesia	20,0347
Sulphate of potash	27,7037
Muriate of soda	0,1910
Red oxide of iron	0,3376

This mineral has been found at Isebel in Austria in the midst of strata of rock salt.—*Journal de Physique.*

STEINHEILITE.

An analysis of the blue quartz of Finland, by M. Gadolin, has shown the principal constituents of this mineral (most unappropriately ranged among the quartz) to be —455 of silex; 230 of alumina; 100 of a particular rose-red matter which cannot be re-

referred to any other known substance; 085 of magnesia; 056 of oxidulate of iron; 074 of water. M. Gadolin proposes to change the name of blue quartz into Steinheilite, as a mark of respect to M. Steinheil, Governor of Finland, who has distinguished himself as a mineralogist, aud was the first to remark that this species should not be confounded with the quartz.—*Revue Encyclopédique.*

A NEW GREEN COLOUR DISCOVERED BY M. BIZIO OF VENICE.

In repeating the beautiful experiment of Brugnatelli on the colouring matter of coffee, I had occasion to observe some new phænomena. When a drop of the infusion or decoction of the grain fell upon a piece of cloth, it formed a yellow spot surrounded with a beautiful green border. I attributed this green colour to the oxidation of the oil of coffee. In order to fix that colour I boiled a hectogramme of coffee powder and reduced the decoction to eight hectogrammes. I added an equal quantity of sulphate of copper dissolved in water, and used as a precipitate a solution of caustic soda. A deposit was formed weighing 105 grammes, which on drying in the air took a green colour; the more it was exposed to the air while it remained humid, the brighter the colour became. Water, ether, alcohol and the alkaline subcarbonates had no effect on the colour. Ammonia indicated the presence of copper; caustic potash changed it to sky blue, and took itself a green colour; caustic soda did not alter it, and received but a slight tinge of the green.

The deposit, which is a true lac, resists acids sufficiently well, and, with the exception of the sulphuric and oxalic, no others destroy the colour totally. Acetic acid in dissolving this lac produces a solution of a much finer green.—*Annales Generales des Sciences Physiques, par MM. St. Vincent, Drapiez et Van Mons.*

The Editor of the *Bibliotheque Physico-Economique*, after announcing the preceding discovery, affirms that twenty years ago a Frenchman named Magnan, of Chaumont (Haute-Marne), had by accident discovered the same colouring property in coffee when surcharged with soda.

GENTIAN.

Some researches into the cause of the bitterness in the root of Gentian (*Gentiana lutea*) have led Messrs. Henry and Caventon to ascertain several important facts with respect to this medicament. They recognised: 1. A very fugitive odoriferous principle. 2. A bitter yellow crystalline substance which they have named *Gentianin.* 3. A matter identically the same as glue.

glue. 4. An oily matter, greenish and fixed. 5. A free organic acid. 6. Uncrystallizable sugar. 7. Gum. 8. A fawn-colouring matter. 9. Wood.—*Journal de Pharmacie.*

RHUBARB.

A cultivator of Rhubarb on a large scale states, that the best means of drying it is to strip it of its epidermis. It is a long operation, but both time and expence are found saved in the end by the promptness and regularity of the drying. Several other persons, who have repeated the experiment, have met with the same results.—*Biob. Phys. Econ.*

GROWTH OF WOOD.

It has been ascertained that wood increases in the following proportion; the first year as 1, the second as 4, the third as 9, the fourth as 15, the fifth as 22, the sixth as 30, the seventh as 40, the eighth as 54, the ninth as 70, and the tenth as 92.

From this it is concluded, that wood ought never to be cut till it is in the tenth year of its growth.—*Biob. Phys. Econ.*

SINGULAR JUNCTION OF TWO TREES.

In the forest of Rousse and commune of Simandre, near Bourg, in France, there are two beeches, which from an extraordinary junction are called *the married pair.* The trees are at the root about four metres (12 feet) distant; their greatest circumference is from twelve to sixteen decimetres, and the diameter of one is somewhat less than that of the other. Both shoot up vertically, but at the height of three metres and half, (10½ feet) the trunk of the one bends over, and, forming almost a right angle, projects itself horizontally into the trunk of the other tree, and becomes completely incorporated with it, without the least appearance of fracture or piecing. From this point the joint trunk rises eight or ten metres (24 or 30 feet), and it is crowned at the summit by a tuft of branches. The united trees present the exact figure of the letter *h*. The inferior part looks like a rustic triumphant arch.—*Biob. Physico Economique.*

BOTANICAL QUERY.

When all trees and even herbs point naturally towards the East, as to the source of light, how comes it that the Cedar of Lebanon should point towards the North? By what chemical cause, by what law of physiology, can this sort of transgression of the natural laws of vegetation be explained?—*Biob. Phys. Econ.*

EGYPTIAN OBELISK.

The *Journal des Debats* gives the following as the version of the inscription on the Egyptian Obelisk lately brought from the Island of Philæ to this country by Mr. Banks. The translator, M. Letronne, says that it contains a Petition from the Priests of Isis, in the Island of Philæ, to Ptolomæus Euergetus the Second:

"To the King Ptolomæus; to the Queen Cleopatra, his sister*; to the Queen Cleopatra, his wife†; the gods of Euergetus, greeting:

"We the Priests of Isis, who is adored in the Abatum‡ and at Philæ, the most mighty goddess. Considering that the Strategists‖, the Epistatists§, the Thebarchons¶, the Royal Registrars, the Commanders of the troops guarding the frontiers, and all others of the King's Officers, who come to Philæ; in short, that the troops which accompany them, and the whole of their suite, compel us to furnish them with abundant supplies belonging to the Temple; the consequence of which is, that the Temple is impoverished, and we run the risk of not having means to defray the regular and fixed expenses, caused by the ceremonies and libations, the object of which is the preservation of yourselves and your children. We supplicate you, most powerful gods, to authorize your kinsman** and epistolographist†† Numenius, to write to Lorchus, also your kinsman, and the Strategist of the Thebaid, enjoining him not to practise such vexations with regard to us, nor to permit any persons whomsoever to do so; to grant us, moreover, letters testifying your decision on this subject, and granting us permission to erect a *Stele* ‡‡, on which we will inscribe the beneficence you have displayed to us on this occasion, in order that this *Stele* may transmit to the remotest posterity the eternal memory of the favours you have granted us. This being permitted us, we shall be, we and the Temple of Isis, in this, as in all other things, your grateful servants. May you be ever happy."

* Widow and sister of Ptolomæus Philometor, afterwards wife of Ptolomæus Euergetus, and repudiated by him.

† Daughter of the other Cleopatra, and of Ptolomæus Philometor; afterwards the wife of Ptolomæus Euergetus, her uncle.

‡ An island near Philæ, consecrated to Isis.

‖ Governors of the Provinces of Egypt.

§ Officers whose functions are not known.

¶ Governors of the whole of the Thebaid.

** An honorary title, similar to that of "Our Cousin," by which the King addresses the chief dignitaries.

†† Secretary of State.

‡‡ The word signifies the obelisk itself, on the base of which the Greek inscription is found.

Ac-

According to M. Letronne, the date of this Petition must have been previous to the year 126 of our era. The object of his Memoir is to extol and explain the various peculiarities which the Greek text presents, to explain the customs to which several passages of the Petition refer, and to form from it some idea of the state to which the cast of Priests was reduced under the domination of Ptolemy. M. Letronne by no means joins in the expectations which have been conceived of the advantages of comparing the Greek text engraved upon the pedestal with the hieroglyphics on the obelisk itself. He seems to think, both from the sense and the object of the Greek inscription, that, if the obelisk is not of a more ancient date, and afterwards restored by the priests of Isis, and consequently, if the hieroglyphics which cover it were really sculptured on this occasion, which seems to him the more reasonable hypothesis, these hieroglyphics contain, in the terms of the Greek text, a testimonial of the gratitude of the Priests to the Princes, and not a second copy, in the Sacred Language, of the Petition inscribed on the pedestal.

THE LATE EXPLOSION AT CORVILLE COLLIERY.

Extract of a letter from Mr. H. Atkinson of Newcastle, to Mr. Riddle of the Royal Naval Asylum, Greenwich.

"You would see in the papers an account of the dreadful explosion which took place in the pit beside Mr. Buddle's, where they have lately begun working the principal seam that lies between the high and low mains. It was not true, however, as was stated, that Mr. Buddle himself went down immediately, he was not there at the time it happened; it was an overman who ventured his own life in endeavouring to save the lives of his companions. You may judge of the quantity of gas which is continually escaping from the coal, from this circumstance: If a hole of about three quarters of an inch in diameter be bored five yards into the coal, it affords a constant supply of gas sufficient to keep up a flame, at the orifice, two inches in length, for about a fortnight. There were several such lights in the pit, and yet the men were working with *candles:* although they not only knew this, but were also aware that the discharge of gas from the surface of the coal, wherever it had been lately exposed, was so great as to keep the air immediately in contact with it almost constantly at the firing point. The result however will surely be a lesson to them not to rely upon ventilation alone, where they have other means of safety, and where the danger is so great."

A FEW

A FEW NOTES ON A SUBTERRANEOUS EXCURSION INTO A LEAD AND SILVER MINE, IN THE PARISH OF ALSTON, IN THE COUNTY OF CUMBERLAND.

On the 19th of February 1818, a party of gentlemen made an excursion in the mine of Hudgilburn, to view a cavern in the limestone rock there, discovered but a short time previous to that date.

At about 4 P. M. being dressed in the working habiliments of the miners, and seated in ore waggons, two in each, *vis a vis*, we were hurled along into the interior region of the mountain of Middle Fell.

We entered the cavern—a light was sent forward, which showed the direction to be in a straight line for a great distance. The light appeared dim, and like a star peeping through a dingy cloud. The width varies from about three to six feet, as I thought, but we did not then measure either the width or the height. The roof has along its centre an indentation the whole length, and its chasm appeared somewhat wider at the top than it is at the bottom; which, with the groove or rent in the middle of the roof, impressed a conception on the mind, of the sides having been thrown to recline backwards by some convulsion of nature. The groove is shallow, and appears like a wound healed up, leaving the scar as a mark of the injury formerly received.

Advancing about half way, we came to a thin rock which divided our passage into two. We pursued the right hand passage, now become so narrow, that a bulky man could scarcely brush through, but widened a little further on. As we passed along, several openings and small recesses on our right and left were seen, but not of a sort to excite much interest, until we reached the far end of this passage, where there is an open space equal to a room of ordinary size, with a beautiful cabin on one side, nearly square, lined with smooth jet black walls, richly spangled with stalactites, that sparkled equal to brilliants of the first water. The solemn grandeur of this place inclined the whole to pause, and contemplate the sublimity of the novel scene around us. We rested on the floor of solid limestone, and gazed on this charm of nature with awe and wonder. When I beheld a scene so superior to what can be produced by all the arts of man on earth, I could not conceal my regret that such treasures should be made so difficult of access, that they should be where—

> " At each step
> " Solemn and slow the shadows darker fall,
> " And all is awful, list'ning gloom around."

The substance of so jet a black with which this charming little cabin is lined, is called by miners " black jack." It contains a por-

a portion of the ore of zinc, and is smelted for its valuable produce in great demand throughout this realm for potteries, medical purposes, brass, &c. In this beautiful little room, there are two openings, in form, nearly square, from the floor upwards, about 1½ foot each side, lined with the same substance, and embellished with glittering spar, of exquisite brilliancy. These transparent particles are very regularly distributed over the walls, neither too thick nor too thin, to give the effect of genuine taste and finish: but the process of nature is going on, and that brilliant spar will most probably become a thick crust, if not impeded by the hand of the workman, and will in time attain to a solid mass of quartz, of which numerous large pieces are found in these mines.

While we rested here, men were sent further in advance, to explore the extent and nature of the several low and narrow passages and openings in the rock, which communicated with this open space; and having taken hold of the end of the clew of pack-thread to direct their retrograde steps by the same way, they tried to advance :—they proceeded on hands and knees, or feet, as necessity dictated, a considerable way forward in the largest openings they could find, until they were called back by the voice and a tug of the line. They found no end to these numerous intersecting openings in the rock, the passages of which are extremely intricate and dangerous, without proper precautions taken; for, to retrace exploring steps in such a labyrinth, if lights should fail, without a clew, or their companions stationed as we were in the main track, would be to hazard their lives.

Our curiosity on that occasion being gratified, we commenced on our return, by the same passage before described, but discovered some other passages that communicated with it, and in which some of our fellow travellers ventured to wander, and were able to join us again, without being obliged to return to the part where they entered the by-way.

The length of the main chasm is 320 yards. Evident signs would seem to prove that this cavern and all its communicating fissures have been filled at no very distant period, with water, and the probability is, it has been drained off by the adits in the mine, in which there runs, as I said before, a constant stream from some contiguous part of the works. The rocks of the cavern are covered by a sooty mucus in nearly a dried state, which it may be presumed, was generated by the stagnant water and impure air, previous to its draining. There is a little mud left on the bottom of the cavern in a moist state, and the smell tends to confirm the conjecture of these concavities having been a reservoir for thousands of years, and drained off by the level of the mine. It appeared to me that some little ventilation passes through the whole,

whole, which might have been so ever since the water was let off; for the air from the level would follow the vent of the stream, and since the opening to the cavern was effected, a slight circulation of air would probably be created.

There were, I think, nine of us altogether; we were in the cavern upwards of half an hour, and we felt no material difficulty in breathing, while our candles, one to each, burnt sufficiently clear; which, with the animal breathing, must together have consumed a very considerable quantity of pure air, such as to have made a scarcity perceptible, if no fresh air had been supplied.—*Newcastle Magazine.*

QUADRATURE OF THE CIRCLE.

M. Scamarella, a Venetian geometrician, announces in the Gazette of Venice of 23d November, that he has solved the problem of the quadrature of the circle, and that he is ready to demonstrate it incontrovertibly to all the mathematicians in the world. According to M. Scamarella, the superficies of a circle is equal to the square of the proportional between the diameter of the circle and a line equal to three-fourths of the same diameter. It is also equal to the square of the circumference multiplied by half the radius, estimating their ratio as 7 to 21, and not as 7 to 22, as Archimedes taught. M. Scamarella further engages to solve all the most difficult problems of this nature, *in faccia a qualcunque Matematico.*—*New Monthly Magazine,* No. 13.

CLOCK WORK MACHINERY.

(From the New York National Advocate.)

There are now exhibiting at Mr. Vogel's in Broadway, several wonderful pieces of clock work machinery, which, perhaps, equal the masterly ingenuity of the automata of Vaucauson, or of Albert the Great.

The first is a small elegantly wrought gold cage, surmounting a musical clock work. In this cage is a fountain, and a bird not larger than a bee, which sings, flutters its wings, and flies from one part of the cage to another. The base of the second is also occupied by a musical clock work; it represents a group of quadrupeds around the basin of a fountain, where a goat drinks, and performs a variety of movements. In front is a basket with a pear in it: the moment the pear is touched, a dog on the other side gnashes his teeth, barks, and shakes himself till the pear is replaced; while a monkey behind threatens him with a stick, and in the mean time munches an apple. A butterfly rests on a pillar above the fountain, and moves its wings and feet. The back ground to this group is a mass of rocks, from among which, now and then, a fox makes its appearance. Above these rocks there is a small patch of blue sky, and the sun turning on his axis, and

and also accomplishing his diurnal revolution. This is a remarkably complicated piece of machinery, none of the figures being more than an inch in length.

The third is a cage, very large and highly ornamented. On the top is a black man who beats time to the chiming of several satyrs and two monkeys, one of whom grins quite ludicrously. But the most wonderful things are two Canary birds that sing the natural notes of these birds, flutter and flap their wings, and spring from one perch to another. In this cage is a fountain, which falls by several stories; and the artificial arrangement of pieces of glass represents so naturally the sound and glitter of falling water, that both the eye and the ear may be deceived.

The fourth is a park with two country seats, out of which come two ladies, who exchange mutual salutations, and bow to the company. Attracted by the sudden flight and song of a bird in a grove beside them, they turn and listen. The bird, not larger than a bee, sings and flutters for some time, and then flies away among the trees. Upon this, the ladies repeat their bows and curtsies to each other and to the company, and withdraw into their houses. On the top of the dome above, is a large butterfly, which closes and expands its wings and moves its feet in a perfectly natural manner. This and indeed all the machinery play a variety of tunes.

The fifth and sixth are two magicians, the French and the American. There is a set number of questions to each; and on any one of these being placed in a drawer for the purpose, the magician goes through a variety of ceremonies and gives the answer, which is always appropriate. It is said that several celebrated mechanicians have been allowed to take these machines to pieces, yet have never been able to discover by what contrivance the right answer is always given.

The last is called a perpetual motion; although perhaps the power that it possesses is not strong enough for any application to extensive machinery. It consists of a large wheel, around the edge of which are placed at equal distances a certain number of moveable hollow cylinders, each containing an equal proportion of quicksilver. The weight of the quicksilver, which moves from one side to the other as the wheel turns, determines the horizontal or perpendicular position of the cylinders. By their horizontal position, in falling, the circumference of the wheel is continually enlarged on one side, and diminished on the other by their perpendicular position in rising; this creates two unequal semicircles, the one more eccentric than the other, and thus causes a perpetual rotation.

FASCINATION OF THE SNAKE.

(From a letter signed *Caroliniensis*, in the New York Columbian.)

A friend in South Carolina, to whom I was on a visit, invited me to a morning walk round his plantation, and recommended our fowling-pieces as companions. The day proved to be very sultry; and while my friend proceeded to give some directions to a gang of his Negroes at a distance, he advised me to take the benefit of a shade formed by a wood adjoining the field in which we then were. I took the hint; and while leaning on the fence, (which was constructed on a bank between two dry ditches,) I was alarmed by the rattle of a snake very near me. I instantly sprung on the top rail of the fence, and the next moment discovered the monster in one of the ditches within ten feet of the spot where I was seated. As I levelled my gun at his head, and was in the act of pulling the trigger, his tail ceased to vibrate. Conscious, from his position, that I was not the object of his regard, and that I was in no danger from him, and confident that I could destroy him at any moment I pleased, I sat still to observe his further movements. As his eyes seemed to be riveted to a particular spot, I followed their direction, and discovered a wood rat. At the moment of my first seeing this little animal, he was rising from a crouching posture, and endeavouring to retire by a retrograde movement. This attempt was immediately followed by a second tremendous exercise of the rattle, and the rat again sunk to the ground. I witnessed several repetitions of this operation; and the result was, that, at length, the rat appeared perfectly exhausted; the snake advanced towards his prey, and was in the act of taking it into his mouth, when I discharged my two barrels at his head, and killed him on the spot. Whether any of my pellets struck the rat, I am unable to say; but, after the closest search, we could detect no mark of violence about his body, and he was dead when I took him up.

Some years after the foregoing circumstance had taken place, as I was accompanying a lady to church in a gig, we were alarmed by the rattle of a snake on the road side. After I had tranquillized the horse, and prevailed on the lady to hold the reins, I returned to the spot from whence the noise seemed to issue, and soon discovered the subject of our alarm. The monster was lying in a coil, ready to strike, but manifested no concern at my approach. Having armed myself with a long fence rail, I was in the act of crushing his head, when I saw a *rabbit* in the very same posture and condition which the rat had exhibited.—The fall of my weapon disabled the snake, and I soon dis-

dispatched him.—The rabbit I took into my hands, without an effort on its part to resist or escape, and deposited it in my companion's lap: but it died before we reached the church. I am confident that the animal had sustained no bodily injury either from the snake or myself.

LAMPYRIS ITALICA.

M. Grotthus being lately at Rome, paid particular attention to the phosphorescent organ of the *Lampyris Italica*. This insect plunged into water, continued luminous for several hours; under oil of olives the light diminished after a quarter of an hour, and disappeared entirely after twenty minutes. The case was nearly the same with hydrogen gas and carbonic acid. When the insect was withdrawn from this gas and transported immediately into an ordinary atmosphere, the phosphorescence recommenced on the instant. Some Lampyræ in which the phosphorescent power was so far extinct that oxygen gas could not revive it, recovered it when plunged into an atmosphere of nitric vapours. When the phosphorescence became extinguished by the nitric vapour, it could no longer be developed by any other agent.—*Grotthus's Forschungen*, 1820.

AN AEROLITE.

On the 15th of June last at three o'clock in the afternoon, and at the same instant when the high mountain called the *Gerbier de Jone*, near Aubenas (department of Ardeche), disappeared and gave place to a lake, a globe of fire which threatened to swallow up the whole village of Berias in the canton of Argentiere (same department) descended perpendicularly upon a smiling valley near Croz, where it left after two strong detonations an aerolite of the weight of ninety-two kile grammes, sunk more than two metres into the ground.—*Bib. Phys. Econ.*

ATMOSPHERIC PHÆNOMENA.

Bamberg, December 25, 1821.

Yesterday, about seven o'clock, in the evening, the sky being clear and serene, there was observed in the neighbourhood of Battenheim and Altendorf an igneous meteor, of a globular form, about the apparent size of a full moon, which, after taking a direction from north-east to south-east, fell to the ground and disappeared, with an explosion as loud as the report of a cannon. Its light was as strong as that of a bright flash of lightning. On the 25th the mercury in the barometer fell lower than had ever been seen by the oldest inhabitants.

[This phænomenon, says a letter from Frankfort of the 31st ult., appears to have been seen at places very distant from each

 other.

other. On the nights of the 24th and 25th the mercury likewise fell at Frankfort to 26 inches six lines, without being accompanied by any other change in the atmosphere but a strong wind, which did not rise to a tempest as in other places. The wind was stronger in the night of the 29th, though the mercury had risen a little.]

TRIGONOMETRICAL SURVEY.

Captain Vetch and Mr. Drummond, the engineer officers intrusted with the conduct of the Trigonometrical Survey in the North of Scotland, have finished their task in Orkney and Zetland, by establishing in those clusters of islands the several positions which serve to connect them with the main land of Scotland. In their operations they were attended by the Protector gun-brig, Captain Hewet commander; and that gentleman was employed at the same time in a nautical survey of various harbours among those islands, which stood in need, particularly in Zetland, of more accurate charts than have yet been given to mariners. The laborious and hazardous task has been brought to a conclusion, with one loss; Mr. Fitzjames, midshipman, and four men, having gone from the rendezvous at Calfsound in Eda, to the island of Sanda for some provisions, were lost on their return, in one of those fearful currents of tide (the Lashy roast), which are frequent among those islands.

MEDICAL AND CHEMICAL LECTURES.

Dr. Pearson's Lectures on Physic will commence on Friday the 8th of February, at No. 9, George-street, Hanover-square, at 9 o'clock in the morning; and Professor Brande will commence his Course of Chemistry in the same week. Pupils to either of the Lecturers are free to both.

LIST OF PATENTS FOR NEW INVENTIONS.

To Julius Griffith, of Brompton Crescent, Middlesex, esq., who, in consequence of discoveries made by himself, and communications made to him by foreigners residing abroad, is in possession of certain improvements in steam carriages, and which steam carriages are capable of transporting merchandize of all kinds as well as passengers upon common roads, without the aid of horses.—Dated 20th December 1821.—6 months allowed to enrol specifications.

To [illegible]e Erard, of Great Marlborough-street, Middlesex, musical-instrument maker, who, in consequence of communications made to him by a certain foreigner residing abroad, is in possession of an invention of certain improvements on piano fortes and other keyed musical instruments.—22d Dec.—6 mo.

To

To George Linton, of Gloucester-street, Queen-square, merchant, for a new method of impelling machinery without the aid of steam, water, wind, air, or fire.—22d December.—6 mo.

To Richard Ormond, of Manchester, Lancashire, iron founder, in consequence of a communication made to him by a certain person residing abroad, for an improvement in the mode of heating liquids in boilers, and thereby accelerating and increasing the production of steam.—7th Jan. 1822.—6 months.

To William Ravenscroft, of Serle-street, Lincoln's Inn, Middlesex, peruke-maker, for his forensic wig, the curls whereof are constructed on a principle to supersede the necessity of frizzing, curling, or using hard pomatum, and for forming the curls in a way not to be uncurled; and also for the tails of the wig, not to require tying in dressing, and further the impossibility of any person untying them.—14th Jan.—2 months.

To Richard Summers Harford, of Ebbw Vale Iron Works in the parish of Aberystwith, Monmouthshire, iron master, for his improvement in that department of manufacture of iron commonly called Puddling.—9th January.—4 months.

To James Harris, of St. Mildred's-court, city of London, tea-dealer, for his improvement in the manufacture of shoes for horses, and other cattle.—9th Jan.—6 months.

To David Loescham, of Newman-street, Oxford-road, Middlesex, musical-instrument maker; and James Allwright, of Little Newport-street, parish of St. Ann, Soho, cheesemonger, in consequence of a communication from a foreigner residing abroad, of a new or improved keyed musical instrument, comprising in itself many qualities never hitherto produced in one instrument, and possessing those qualities in clearness of sound, quality, distinctness, forte piano, delicacy of touch and shake on the keys or notes by increasing to forte, and decreasing to piano at the will of the performer.—14th Jan.—6 months.

To Alexander Gordon, of the city of London, and David Gordon, of the city and county of Edinburgh, esquires, for certain improvements and additions in the construction of lamps, and of compositions and materials to be burned in the lamps, and which may also be burned in other lamps.—14th Jan.—6 mo.

To David Gordon, of the city and county of Edinburgh, esq., for certain improvements and additions to steam packets and other vessels, part of which improvements are applicable to other naval and marine purposes.—14th Jan.—6 months.

To Augustus Applegath, of Duke-street, Lettsom-town, Lambeth, Surrey, printer, for certain improvements in printing machines.—14th Jan.—4 months.

BARO-

BAROMETRIC OBSERVATIONS.

Arundel, Jan. 15, 1822.

SIR,—I send you the Barometrical Observations made at this place on November 12th, December 10th, and the 14th instant.

Your obedient servant,

To Dr. Tilloch. G. CONSTABLE.

1821.	Barom.	Thermom. att.	Thermom. det.	Wind.	Weather.
Nov. 12th.					
8h	29·890	55·5	55·0	S.W. calm.	Fair.
9	29·915	55·5	55·0	S.W. do.	Do.
10	29·920	56·0	55·0	S.W. do.	Do.
11	29·922	56·0	55·5	S.W. mod.	Cloudy.
12	29·925	56·5	56·0	S.W. do.	Fair.
P.M. 1	29·928	57·0	56·5	S.W. fresh.	Do.
Dec. 10th.					
8	30·020	52·0	51·5	S. fresh.	Cloudy.
9	30·020	52·0	51·5	S. by W. do.	Do.
10	30·020	53·0	53·0	S. by W. do.	Do.
11	30·015	53·[illegible]	53·0	S. do.	Do.
12	29·992	53·5	53·0	S. do.	Do.
P.M. 1	29·985	54·5	54·0	S. do.	Do.
Jan. 14th.					
1822. 8	30·293	48·5	48·0	W. calm.	Fair.
9	30·305	48·5	48·0	W. do.	Do.
10	30·320	48·0	48·0	W. mod.	Do.
11	30·332	48·0	48·0	W. do.	Do.
12	30·325	48·5	48·0	W. fresh.	Do.
P.M. 1	30·312	48·5	48·0	W. mod.	Do.

Croom's Hill, Greenwich, Dec 31, 1821.

SIR,—With my last register of 1820, I mentioned that less rain had fallen that year than for several preceding, and that a want of water had been felt both by mills and canals. A similar observation will not be applicable to the year past, for the register now sent shows a very great excess to the quantity of London and its neighbourhood, beyond many former years. The evaporation has been rather less than the average of the last four years. It may be worthy of remark, that the rainy and most tempestuous weather in the two last months has been of wide extent, and that the great storm about Christmas, when the barometer was so low as 28·07 (say twenty-eight inches and seven hundredths), was very disastrous both here and in the Mediterranean, and that earthquakes occurred in Bavaria and other places. As the different heights of rain gauges above the contiguous ground on which they are placed, has a material effect

on the quantity of rain caught, it would be well if every person favouring the public with their observations would with every register mention the height at which their instruments were placed above the ground. I remain, sir, your obedient servant,

To Dr. Tilloch. HENRY LAWSON.

P. S. Having made atmospheric electrical observations, both day and night, with an exploring wire (sixty yards in length) from the 24th to the 26th of December, during the time the barometer was so low, I found all the electrical indications were constantly negative.

Height of Rain Gauge and Evaporator above the Ground—Four Feet.

Months.	Rain.	Evaporation.	Months.	Rain.	Evaporation.
1821.			1821.		
From			Jul. 8 to 15	0·155	0·653
Jan. 1 to 7	0·574	0·225	15 to 22	0·417	0·758
7 to 14	1·668	0·041	22 to 29	0·687	0·803
14 to 21	0·386	0·081	29 to 5 Aug.	0·238	0·760
21 to 28	0·055	0·041	Aug. 5 to 12	0·677	0·789
28 to 4 Feb.	0·018	0·156	12 to 19	0·359	0·589
Feb. 4 to 11	0·004	0·139	19 to 26	0·057	0·927
11 to 18	0·000	frozen	26 to 2 Sept.	1·536	0·550
18 to 25	0·045	do	Sep. 2 to 9	0·170	0·487
25 to 4 Mar.	0·768	0·213	9 to 16	1·045	0·374
Mar. 4 to 11	1·055	0·258	16 to 23	0·741	0·468
11 to 18	0·216	0·336	23 to 30	0·389	0·357
18 to 25	0·354	0·344	30 to 7 Oct.	0·949	0·403
25 to 1 April.	0·818	0·423	Oct. 7 to 14	0·298	0·203
Apr. 1 to 8	0·435	0·503	14 to 21	0·424	0·134
8 to 15	0·637	0·544	21 to 28	0·738	0·116
15 to 22	0·601	0·449	28 to 4 Nov.	0·561	0·167
22 to 29	0·053	0·857	Nov. 4 to 11	0·059	0·107
29 to 6 May.	0·059	0·645	11 to 18	2·230	0·158
May 6 to 13	0·081	0·614	18 to 25	0·668	0·108
13 to 20	1·492	0·613	25 to 2 Dec.	0·987	0·207
20 to 27	0·229	0·540	Dec. 2 to 9	0·979	0·079
27 to 3 June.	0·138	0·843	9 to 16	0·090	0·076
June 3 to 10	1·859	0·597	16 to 23	1·229	0·166
10 to 17	0·294	0·539	23 to 30	2·477	0·107
17 to 24	0·003	0·683			
24 to 1 July.	0·194	0·850	Total Inches.	31·143	20·507
July 1 to 8	0·947	0·427			

Rain. There fell in			Evaporation.
1817	25·349	1817	22·227
1818	24·252	1818	27·064
1819	27·339	1819	21·369
1820	23·274	1820	19·621
1821	31·143	1821	20·507

1821.		Barom.	Ther. attach.	Ther. detach.	Wind moderate.	
Dec. 10.	8h	29·308	51·5	51·	S.W. blowing down an inclined plane from the house.	Cloudy.
	9	29·314	52·	51·5		
	10	29·300	52·	52·	S. blowing freely to and past the house.	
	11	29·278	53·	53·		
1822.						
Jan. 14.	8	29·750	46·5	40·	S.W. blowing, &c. rather [strong.	Flying showers, but mostly bri.
	9	29·772	52·5	41·	Do.	
	10	29·780	54·	42·	Do.	
	11	29·788	54·5	42·	W. Do.	
	12	29·772	59·5	42·5	Do.	
	1	29·770	55·	43·	Do.	

SIR, Hafod, near Mold, Flintshire, Jan. 16, 1822.

The above are the heights of my Barometer at the specified times. But I beg leave to observe, that neither these nor my observations in February last were corrected by the fraction marked on the Barometer, namely, $\frac{1}{66}$, for I conceive that the difference is too minute to be regarded until the general operation of the instrument is found to be less varied with reference to the desired object.

It will be seen that there is a very material difference in the comparative heights of my Barometer, and those of your other correspondents in February and December. In the former month the prevailing wind (which was very moderate) came up the Vale of Mold almost directly against the front of my house, which is sheltered by a plantation of 27 years growth, and of considerable extent, on a hill at a short distance behind it; and at that time the average height of the mercury in my Barometer appears to have been very nearly the same as in that of Col. Beaufoy at Bushey Park. Whereas in December, with the wind from an opposite quarter, mine appears to have been lower by ·245, though with a higher temperature.

Is it possible that from this cause the atmosphere here in February may have been *locally condensed?* Allow me to throw out this hint, as I conceive that experiments might be made to ascertain whether this is the case under such circumstances, but which I have not leisure at present to attempt. At any rate, it appears to me to be desirable that each communication should be accompanied by a statement of the situation of the instrument with reference to the adjacent country or buildings, &c. as opposed or otherwise to the prevailing winds at the times of observation.

I am, sir,

Your most obedient servant,

WM. WARD.

P.S.

P. S. Upon a hasty observation of the winds that have prevailed in different months, with reference to the greatest discrepancies, such as Crumpsall and Leighton in June and August, Manchester and Leighton in April and June, &c. &c. these winds appear to me to have been always from opposite quarters. Your correspondents will be able to appreciate the probable effects of the variation of the wind at their respective stations.

The following Barometrical Observations for 1821, taken at 10 o'clock daily, were made by Mr. R. Webster, Cornhill, London.

	Inches.		Inches.
January .. .	30·229032	July	29·942193
Maximum .	30·95	Maximum ..	30·225
Minimum .	29·125	Minimum ..	29·65
February .. .	30·21160	August	29·937096
Maximum .	30·65	Maximum ..	30·15
Minimum .	29·35	Minimum ..	29·55
March .. .	29·679932	September	29·8425
Maximum .	30·30	Maximum ..	30·20
Minimum .	29·15	Minimum ..	29·45
April .. .	29·655833	October	29·901774
Maximum	30·05	Maximum ..	30·255
Minimum	29·35	Minimum ..	25·20
May .. .	29·866935	November . ..	29·830833
Maximum .	30·20	Maximum ..	30·25
Minimum .	29·25	Minimum ..	29·35
June ..	30·15	December ..	28·880
Maximum .	30·25	Maximum ..	30·20
Minimum .	29·65	Minimum ..	28·35

The mean atmospheric pressure of the whole year	29·833236
The maximum of the year	30·95
The minimum of the year	28·35
The barometrical range	2·6

The elevation of the place of observation (as measured by a capital mountain barometer by Mr. Jones, taken at the mean of several observations,) at 60 feet above the level of the sea; the latitude at 51 deg. 30 min. 38 sec. North; the radius of the earth considered 3954·590 miles.

N. B. The elevation is particularly given, [illegible] force of gravity increases inversely as the square of the distance from the earth's centre.

Results of a Meteorological Register kept at New Malton, Yorkshire, in 1821.

1821. Months.	Barometer. Mean.	Max.	Min.	Range.	Spaces described in Inches, &c.	No of Changes.	Thermometer. Mean.	Max.	Min.	Range.	Winds. N	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Var.	Brisk.	Boist.	Weather. Hail.	Snow.	Rain.	Rain, &c. Quantity in Inches, &c.
January	29·704	30·88	28·62	2·26	6·21	12	36°·840'	52°	20°	32°	6	6	1	0	4	8	3	3	0	2	2	0	2	5	1·54
February ...	30·110	30·64	29·07	1·57	6·33	17	35·160	49	20	29	4	4	3	0	0	6	8	1	2	1	0	1	1	1	0·26
March	29·297	30·22	28·70	1·52	11·05	17	41·115	53	29	24	3	1	3	2	4	11	4	3	0	7	4	1	2	13	3 40
April	29·366	29·98	28·33	1·65	9·31	18	49·683	69	33	36	2	5	1	0	8	5	7	0	2	2	3	4	0	10	2·08
May...	29·649	30·15	28·80	1·35	6·14	16	49 370	68	32	36	9	3	1	2	3	1	8	0	4	2	1	4	2	13	3·04
June...	29·970	30·38	29·45	0·93	3·25	16	53·400	71	40	31	18	8	0	0	0	0	1	0	3	0	0	0	0	4	0·80
July...	29·687	30·18	29·10	1·08	6·00	14	58·200	73	38	35	6	5	2	0	4	5	5	2	2	2	1	0	0	15	2·28
August	29·700	30 08	28·84	1·24	4·75	14	61·112	78	40	38	2	6	2	2	3	9	4	1	2	2	0	0	0	12	1·80
September ...	29·533	30 04	28·80	1·24	6·61	20	57·333	72	44	28	0	3	0	2	8	[illegible]	6	2	2	7	2	0	0	14	2·88
October	29·631	30·21	28·40	1·87	8·64	14	49·258	63	37	26	2	0	1	1	4	19	2	1	1	2	2	0	0	10	2·26
November ...	29·394	30·20	28·45	1·75	14·33	24	43·733	59	30	29	2	0	0	2	12	9	4	0	1	8	10	0	0	15	3·75
December ...	29·005	30·20	27·38	2·89	14·38	19	39·693	51	30	21	1	1	1	4	10	4	7	2	1	9	9	0	0	19	5·37
Annual Means, &c.	29·587	30·88	27·38	3·50	97·60	201	47·908	78	20	58	55	42	15	15	60	84	39	15	20	44	34	10	7	131	28·96
For 1820.	29·647	30·90	28·05	2·85	80·95	169	46·900	85	7	78	39	52	26	14	54	84	39	37	21	32	16	7	20	118	29·43

ANNUAL RESULTS.

Barometer.

			Inches.
Highest observation, January 23d.	.. ..	Wind N.	30·880
Lowest do. (continuing 14 hours) Dec. 26th.		S.	27·380
Range of the mercury	.. ..		3·500
Mean annual barometrical pressure	.. ..		29·587
Greatest range of the mercury in December	.. ..		2·820
Least do. do. .. June	.. .. ..		0·930
Mean annual range of do.	.. ..		1·606
Spaces described by do.	.. ..		97·600
Total number of changes in the year	.. .. ..		201·000

Six's *Thermometer.*

Greatest observation, August 23d.	..	Wind S.E.	78·000
Least do. January 2d and 3d. and February 26th.		N. E.	20·000
Range of the mercury in the thermometer	.. ..		58·000
Mean annual temperature	.. ..		47·908
Greatest range in August	.. ..		38·000
Least do. December	.. ..		21·000
Mean annual do.	.. ..		30·416

Winds.

		Days.
North and East	.. ..	70·000
North-East and South-East	.. ..	57·000
South and West	.. ..	119·000
South-West and North-West	.. ..	99·000
Variable	.. ..	20·000

Rain, &c.

		Inches.
Greatest quantity in December	.. ..	5·370
Least do. February	.. ..	0·260
Total amount for the year	.. ..	28·960

Observations.

Pressure.—The most prominent features which present themselves, and the most worthy of remark, are, the great elevation of the Barometer in January, and its unprecedented depressions in December, the greatest of which, and the minimum for the year, occurred near midnight on the 24th,* and continued until 2 P. M. the 25th, attended with a most violent gale from the South; thunder and lightning, and torrents of rain amounting with what had fallen the previous night to nearly *three* inches. On the 29th, the Barometer again fell to 27·73, after which it rose rapidly. From the 16th to the 31st it never attained 29·00, though the changes in its direction were almost daily, and frequently considerable.

Temperature.—The mean annual temperature, which is one

 degree

degree above that of the preceding year, and is owing to the mildness of the autumnal and winter months, fully compensated for the decrease from the usual averages experienced in May, June, and July, which were the only months below the means of the corresponding periods in 1820.

Wind.—The prevailing winds are again S.W. and W. The North and Southerly ones are nearly equal, and the N.W. and S.E. exactly so. The strongest winds have blown from the South, and particularly towards the close of the year.

Rain.—The amount of rain, which has annually and gradually decreased since the wet year 1816, is less than that of the preceding one, though the two last months have nearly brought up the usual average. If the rain be taken from the last quarter of the moon, commencing the 16th ult. up to the same time of the present period (the 15th), the total amount exceeds six inches and a half,—a most unusual quantity for these parts.

New Malton, Jan. 15, 1822. J. S.

Dear Sir,—Having for a considerable time past kept a Meteorological Journal at this place, I beg leave to transmit my last year's table, &c. for insertion in the Philosophical Journal. The instruments made use of are the best I could procure in London, except the rain-gauge, for which I am indebted to Luke Howard, Esq. and the register is kept with great exactness.

Your most obedient servant,

New Malton, Jan. 15, 1822. Jas. Stockton.

To Dr. Tilloch.

The following account of the quantities of Rain which has fallen in each month, in the years 1820 and 1821, is furnished by a gentleman residing in St. Thomas's, near Exeter, in which parish the account was kept:

1820.	Inches.	1821.	Inches.
January	3·68	January	2·53
February	1·38	February	0·32
March	1·84	March	4·49
April	1·44	April	3·43
May	2·23	May	3·06
June	0·57	June	1·26
July	1·05	July	2·98
August	2·17	August	2·38
September	2·42	September	3·10
October	5·68	October	3·36
November	1·62	November	5·44
December	2·49	December	8·56
	26 inches 57-100dths		41 inches 58-100dths

Jan.

Jan. 22, 1822.

Dear Sir,—Having sent the calculated results of the observations on the Barometer in my last, up to November, and having completed the year 1821, as to the monthly observations, I consider it due to yourself and correspondents to acknowledge the obligation I feel for the attention shown to the subject proposed by me; and to assure you that I shall feel great pleasure, at some future period, in renewing the course under some improvements, and hope to be able to fix the *zero* by a permanent mark, in one or more convenient places in London; whence, by means of a revised section of the Grand Junction Canal, extend the line of determined altitudes over a large district. The connexion of other canals, when their sections have been revised, will carry the line of known altitudes to nearly all the towns of importance in the country. The intermediate places may afterwards be determined with considerable accuracy, by taking short distances and proper states of the atmosphere.

It is highly to be regretted that the heights determined by the late Col. Mudge and others, in the great National Trigonometrical Survey, cannot be depended on. I have been at the trouble of levelling, to determine the relative heights of several near the borders of the Grand Junction Canal, and am sorry to find a variation of 20, 30, and 40 feet from the heights published in the Survey. Considering the importance of some of the principal stations, particularly those used in ascertaining the relative length of degrees in the different sections of the English arc, it would not be unworthy of the Honourable Board of Ordnance to correct these heights by actual levelling: the necessary time and expense would be very small.

In your last Number are two months observations at Crumpsall and Manchester, and one at Pocklington, the calculated heights of which relative to Leighton I beg leave to send, in addition to those of last month, viz.

Crumpsall above Leighton.		Manchester *above* Leighton.	
November	187 feet.	November	8 feet.
December	247	December	86.

Pocklington *above* Leighton.

December 11 feet.

To Dr. Tilloch. Yours truly,

B. Bevan.

P. S. In last month's letter I omitted to say Mr. Cary's barometer was *below* Leighton.

METEOROLOGICAL TABLE

Extracted from the Register kept at Kinfauns Castle, N. Britain. Lat. 56° 23′ 30″.—Above the level of the Sea 129 feet.

1821.	Morning, 10 o'clock. *Mean height of* Barom.	Ther.	Evening, 10 o'clock. *Mean height of* Barom.	Ther.	Mean Tempr. by Six's Ther.	Depth of Rain. Inch. 100	N° of Days. Rain or Snow.	Fair.
January.	29·791	37·645	29·780	36·903	37·225	3·20	14	17
February.	30·131	40·750	30·124	38·928	40·357	0·60	7	21
March.	29.465	42·096	29·425	39·774	41·290	3.50	18	13
April.	29.510	49·366	29·503	45·200	[illegible]·366	3·35	16	14
May.	29·768	50·193	29·758	44·935	[illegible]·838	1·70	15	16
June.	30·779	56·666	30·112	50·866	54·800	0·50	6	24
July.	29·784	59·161	29·786	54.709	58·419	1.10	12	19
August.	29·802	59·612	29·800	55·222	59·290	1·15	9	22
September.	29·642	57·366	29·630	54·066	56·666	2·10	16	14
October.	29·654	48·967	29·647	47·580	49·000	1.75	14	17
November.	29·463	43·233	29·487	41·100	42·633	5·25	20	10
December.	29·176	40·290	29·178	39.935	40·290	4·80	25	6
Average of the year.	29·747	48·779	29·686	45·768	47·931	29·00	172	193

ANNUAL RESULTS.

MORNING.

Barometer. *Observations. Wind.*		*Thermometer.* *Wind.*	
Highest, 23d Jan. W.	30·74	6th Sept.	67°
Lowest, 25th Dec. W.	28·14	3d Jan. SW.	20°

EVENING.

Highest, 22d Jan. NW.	30·69	3d Sept. SW.	62°
Lowest, 25th Dec. W.	28·12	4th Jan. NE.	21°

Weather.	*Days.*	*Wind.*	*Times.*
Fair . . .	193	N. and NE.	10
Rain or Snow	172	E. and SE.	132
		S. and SW.	45
	365	W. and NW.	178
			365

Extreme Cold and Heat, by Six's Thermometer.

Coldest, 3d Jan. . . . . . Wind SW. . . . . . 12°
Hottest, 23d August . . . . Wind SE. . . . . 74°
Mean Temperature for 1821 . . . . 47° 9315

RESULT OF TWO RAIN GAUGES. In. 100

Centre of Kinfauns Garden, about 20 feet above the level of the Sea . . . . 21·18
Kinfauns Castle, 129 feet, ... 29·00

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE,

BY MR. SAMUEL VEALL.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1821.	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Dec. 15	21	49·5	29·[illegible]	Fine
16	22	53·5	29·50	Ditto
17	23	50·	29·25	Ditto
18	24	48·	28·82	Cloudy—lightning at night.
19	25	46·	28·82	Ditto
20	26	40·	29·15	Fine—heavy rain at night.
21	27	43·	28·75	Stormy—rain A.M.
22	28	44·5	29·10	Cloudy
23	29	40·	28·80	Rain
24	new	43·5	28·65	Fine—heavy rain A.M.
25	1	41·	28·15	Stormy—Ditto A.M.
26	2	38·	28·40	Cloudy—sharp frost this morning, being the first this season, and rain at night.
27	3	41·	28·80	Cloudy
28	4	42·	28·50	Ditto—stormy with rain P.M.
29	5	45·	28·33	Ditto
30	6	43·5	29·02	Rain
31	7	42·5	29·75	Fine
1822.				
Jan. 1	8	41·5	29·44	Cloudy—rain A.M.—stormy with rain P.M.
2	9	38·5	29·45	Ditto—stormy with rain A.M.
3	10	39·5	29·60	Ditto—rainy morning.
4	11	38·	29·27	Ditto—ditto.
5	12	37·	29·80	Fine
6	13	36·	29·95	Ditto
7	full	37·	29·85	Ditto—snow A.M.
8	15	36·	29·95	Cloudy
9	16	42·	30·	Ditto
10	17	42·5	29·90	Ditto
11	18	48·	30·	Ditto
12	19	48·	30·08	Ditto
13	20	50·	30·	Ditto
14	21	46·	29·95	Stormy

N. B. The Mercury in the Barometer was lower at this place on the 25th December last, than on any day the last six years.

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand.

Days of Month. 1821.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Weather.
Dec. 27	38	47	47	29·02	Stormy
28	44	38	46	28.54	Stormy
29	46	46	45	·54	Cloudy
30	42	44	39	29·[illegible]	Rain
31	36	43	37	·95	Fair
Jan. 1	40	45	36	·68	Showery
2	35	42	34	·70	Fair
3	34	38	37	·72	Fair
4	37	38	35	·26	Rain
5	34	38	34	·90	Fair
6	34	38	38	30·05	Fair
7	34	37	35	29·99	Rain
8	34	38	38	30·13	Fair
9	39	44	36	·19	Fair
10	34	43	42	·14	Cloudy
11	42	47	46	·28	Cloudy
12	43	47	46	·35	Cloudy
13	47	48	45	·34	Cloudy
14	43	47	40	·30	Fair
15	40	42	33	·23	Fair
16	32	37	30	·17	Fair—snow in the night.
17	31	37	34	·17	Cloudy
18	32	43	42	·27	Fair
19	42	45	45	·44	Cloudy
20	46	50	45	·23	Cloudy
21	45	47	46	·37	Fair
22	41	47	43	·51	Fair
23	42	47	46	·28	Cloudy
24	45	47	47	29·91	Rain
25	46	48	44	30·05	Fair
26	44	47	35	·13	Fair

N.B. The Barometer's height is taken at one o'clock.

Observations for Correspondent who observed the

14th Jan. 8 o'Clock M.	Barom. 30·312	Ther. attached 52°	Detached 42
— — 9	— — — — ·316	— — — 50	— — 43
— — 12	— — — — .314	— — — 50	— — 47
— — 1	— N. — — ·302	— — — 50	— — 47

XVII. *On the Absurdity of burying Weeds and turning-in young Crops with the Intention of making them serve as Manure. By Mrs.* AGNES IBBETSON.

To Dr. Tilloch.

SIR, — To establish facts upon the sure and solid foundation of repeated experiment, and to discard all those customs that are derived from too hasty conjecture, and which have not been subjected to proper trials and strict examination, is the duty of every botanist and agriculturist. That such an error as burying weeds and turning-in young crops for the purpose of making manure, should have been maintained in regular practice, without any rational person considering that it was admitting the grossest contradiction in practice, if not in words, is most strange. We wish to keep our roots, such as carrots, turnips, potatoes, free from decay till we want them: for this purpose we place them in the earth; cooks and others having experienced that placing venison in the soil will either freshen it, or at least stop its further progress towards decomposition. These various trials, therefore, prove the earth to possess the power of repelling putrefaction. How then can we in the same manner, and at the *same season*, turn-in our weeds, and the refuse of our fields and gardens, and expect by *this means to procure* for that crop just put in, manure that will nourish and support it? Is that not pretending that the earth will preserve and at the same time decay? Can it do both? If the crop requires manure, is it not deceiving ourselves to turn in that matter which will not produce it? If the positive proof we have received, both in the animal and vegetable world, of the earth's preserving powers, does not suffice to convince us, *a trough is easily procured.* I tried two for three years. We know that potatoes, &c. are not only preserved, but that their roots *grow*, and that the plants throw up suckers and new shoots: on examining the trough, I found that the grass and weeds had repeatedly spread suckers through the top of the case, which proved that they were still perfectly alive. Sir H. Davy (that great luminary of the physical and chemical world) has said that vegetables do not produce manure that can be serviceable to a crop; which, when we consider the process the plants must pass through after death, before they can be sufficiently decomposed to serve as manure, is completely exemplified. There are always *three fermentations* succeeding death, and each takes a long time; the *saccharine*, the *vinous*, and the *putrid*. It is easy to know the different states in which the vegetable *is* at the time of examination,

since the first is always attended with a sweetish taste and clammy feel; the vinous has generally a sour acid smell; and the last will be known by its unpleasant effluvia. All these processes I never saw take less than two or three years. How then can they be serviceable to a crop of wheat that is only in the earth a few months?

It always appeared to me that there was a strange confusion by botanists and gardeners in comparing fresh vegetables, or plants but just dead, with dung, as if *they both* passed through the same process when replaced in the earth. Dung has already been exposed to a very high temperature, to the effects of the gastric juice in the stomach of the animal; and therefore enters the earth after it has undergone each separate fermentation. No wonder it is of such general use, since it is capable of being directly applied to the service of supporting the plants. But how different is the situation of vegetables just cut or drawn up by the roots, and then replaced in the earth! They are not even dead. After keeping one trough closed for near three years, in which I had placed boughs of trees and herbaceous vegetables; and another, in which were weeds and indigenous plants; most of the latter *grew up again*, and many made their way through the top of the trough,—but in the first the bark of the boughs was alone destroyed: no other part was touched, merely dirtied. What is most curious, several of the shoots had formed fresh buds in the earth, but perfectly without scales; which accords with the early decay of the bark.

Another custom almost equally fatal to the farming system, is planting green crops, letting them grow for a time, then ploughing them in as manure for the crop of wheat the following season. When the corn has been reaped, perhaps, and two green crops taken after them, I have secured the one turned in to make manure, and found that the leaves were often eaten by vermin, but no other part in the least decayed; it had drawn around snails and worms,—but that I suppose was not the advantage to be reaped from the insertion of the plants. In the savannas of America, or the woods in the back settlements, I doubt not that the trees dropping where they grew, and having a century or two to assist in their decay, may at last form that black mould which may be of service to plants. But what a mixture each season must it make with atmospheric juices, with rains and dews, ere this can be effected! How many adventitious and accessary circumstances must this length of time produce, before it can complete the decomposition and its reformation! The fat mould of New Holland is almost too rich for the common plants of England and Scotland, and for most wheats; nay, in this country, in places where cultivation is yet little practised, provided much

much wood falls there, 'tis often discovered in nearly the same state. It is also possible by this means to find the woods cut down by the Romans, when they wished to drive the great Caractacus and the Britons back, and not leave them the shelter they sought there. It is certain a very great expense would be spared the farmer, and he would soon find his fields by degrees grow clean, when he had three or four times taken out all his weeds without returning them to the earth, to burn them alone (not paring and burning); though they will make but a very few ashes, still that little quantity may be of use: and they would soon find what an expense would be spared. The greater part of pernicious weeds are only to be killed by manuring; drawing them from the earth by *ploughing* often only increases the number. A gentleman in this county had a lawn so overgrown with Colt's-foot, that he ploughed four or five times, and each ploughing increased the quantity; at last he was persuaded to give it a thorough dressing of dung, of short muck, but turned in hot from the cart. It completely killed the weeds, and he had as fine a field of *Red Clover* as I should ever wish to see.* *Sonchus palustris*, which had overrun a lawn adjoining the E c, was entirely destroyed so as not to appear again, by manuring a few times with rather hot lime.

I never found any manure that would kill Dock without taking it out first with the extractor and filling the holes with quick-lime; then waiting to put in the crop, till this had been done several times, that the hot lime might not injure.

Sir H. Davy has informed us of lime, "that it is in its passage from quick-lime to carbonate of lime it is capable of decomposing vegetables;" but he expresses himself as if not quite certain of the fact. It was however on this opinion I have acted, and founded my various trials. I first placed different sorts of meat in a small trough on mild lime, and covered it with the same matter. Its effects were most curious: the lime formed a cake near two inches in thickness around the meat, which appeared to shut out all air, as it was perfectly dry and hard. The manner in which animals and vegetables decay is very different; the first undoubtedly forms a vacuum, and thus preserves it. In the case of vegetables the lime is always perfectly loose, and not in any manner coagulated; but to my great astonishment they were so much decayed even in their wood and muscle (the hardest part) as to promise total decomposition, if lime had been once more applied.

Next to banishing weeds, the most important subject to common agriculture, is to ascertain most positively how soon and in what state lime will decay the woody plants or trees; and enable them to return (if they ever do so) to that *situation* which will fit them to be of service as manure to other plants. This is certainly only to be *effected* by their becoming *earth* *again.*

again, or at least a sort of decaying matter, nearly allied to a kind of fat mould which we sometimes get from an earth that has not been broken up for a vast time, but that if suddenly put into tillage gives a very great return. When I opened the trough the second time, the wood was certainly more decayed than ten years of earth would have effected; and I found some black greasy powder rather too fine for earth, but much like it. If three dressings of tolerable hot lime will bring into agricultural order our commons loaded with furze and *Erica*, it would certainly answer in point of expense, especially as the upper layer might be carbonate at the third time of manuring, which would bring a fine bed of Cow Clover.

It is quite painful to see to what extraordinary expenses farmers will expose themselves to *ruin their land*. Whatever is the fashionable manure in the neighbourhood, whether it is *lime*, *dung*, *marl*, *ashes*, or *clay*, though perhaps the soils may be as various as our climate; still the same nutriment is applied. It is very common to see one manure placed on another which renders it perfectly nugatory, as lime on dung, lime on lime. In following the labours of a farmer, especially in Devonshire, you would think it was quite enough to dirty straw to make it manure. I have often seen straw thrown at a great expense in roads where no horses passed, the straw serving merely to tie the lumps of earth together, that when the frost came it might become impossible to break or decompose them. If nutriment is necessary to plants (and few will deny so obvious a proposition), it must be that nourishment which can assimilate with their juices, the quality of the vegetable planted, and the manure must be applied accordingly. Nutriment is given in three ways; first, spread or scattered; secondly, laid into drills as a sort of top dressing; the third is by the drill-machine with long dung, which does best for wheat, especially in clay soils. But each must not only be adapted to the plant which is going to be placed in the ground, but the plant must be also suited to the soil, and the manure must be suited to both. It is a well known fact, that we are the only people who thus ruin their agriculture by being indifferent to this subject.

There are but few agricultural plants usually made use of by a farmer; of these I selected *twenty* to try in a variety of soils to endeavour to ascertain in which ground each would yield the greatest return, looking to profit. The turnip and carrot gave three parts in twenty more in sand than in any other soil. The cabbage two parts more in clay than in any other earth. The immense difference maintained by the Saintfoin in chalk or limestone, never proved less than an increase of four in twenty: Hops showed a predilection nearly as great. The Cow Clover was equally

equally decided in its choice of poor sand. The Mangel evinced its superiority $2\frac{1}{2}$ in clay. As to the wet and dry clovers, to change their soil is to destroy them. In the wheats, to mistake the soil for which they are intended is to blight them more or less each year. I have been constantly able to banish and bring on the blight by this means. There are several wheats for each sort of land, so that the identical plant may be changed as often as is necessary. The experiment of the farming plants tried three years successively, and the quantity of manure given them, though differing in quality, was as nearly as possible the same in measure.

How is it possible to conceive that the same manure can suit each soil? A hot gravel which wants cooling, moisture, and nutriment; a cold stiff clay which requires dispersion, pulverizing, dryness and support; a rich earth which calls only for lime to reduce its acidity; or a poor sand that demands binding, nourishment and moisture; a limestone which, like the clay, wants pulverizing, support and warmth. The hot gravel is more admirably manured with marl and ashes: the clay after drying is best assisted with sand and good rotten dung, which divides, warms and supports it: a rich earth wanting only lime to reduce its acid: the poor sand, marl, or clay and chalk mixed with long dung.

Thus there is a sort of law by which the most unlearned or careless farmer might plant his vegetables, put in his corn, accommodate his various plants according to their soil; in short, make good husbandry by thus proceeding by rule, and by means of the most trifling trials comprehend the earth of which each field was composed. I taught several farmers the method of attaining this simple piece of knowledge without trouble or expense. Thus, if the stiffness of a clay is to be ascertained, by way of properly adapting its manure, which is absolutely necessary, form a small bason of it, and pour in a cup of water: the time the water is passing off will at once show its strength by its retentive qualities, and of course the nutriment it requires. If marl, the quantity of acid necessary to saturate and decompose it will show its goodness, since Sir H. Davy says that 50 per cent. of lime is sufficient to make it tolerable good manure; if 60, as some I have found at Exmouth, it is very excellent marl; the rest is a sort of soapy clay, which adds to its value. If sand, if the soil is washed in four times its weight of water, the sand will subside at bottom and show of what else it consists. How often is clay found as a subsoil to sand! If it was therefore examined, it is only mixing and adding long dung, and the whole becomes compounded for many plants. How often does a farmer go to great expense to procure that which he would find in his next field! If the farmer tries his sand, it is necessary he should

should know of what species it is, that he may manure accordingly. If siliceous, it will scratch glass: if calcareous, the acids will show it: if aluminous, it is so soft that it will cut with a knife, the small stones are so soft. All these things are of consequence for a farmer to know; and a small pamphlet of this might be useful, with the trial of the rest of the twenty plants.

I must mention, before I close this letter, that my weeds were all marked before they were placed in the trough with coloured threads, by which means I knew them again when growing up. I cannot think it a fair trial without the matter said to have been weeds is taken out and examined, to show in what state *it is*, whether really capable of manuring plants, or not. A gentleman, after letting his gardener rake out the hole, showed me earth which he declared *had been weeds* two months before; I only insisted on seeing the raked matter, and all the weeds which had not grown up were in it in a half-dying state: but in woody plants the folly is still more complete, and in turning-in young crops of beans or vetches, except the leaves that are eaten by vermin, all the rest remains perfect, take it out when you will.

XVIII. *On the Separation of Iron from other Metals.* By J. F. W. Herschel, *Esq. F.R.S.**

An easy and exact method of separating iron from the other metals with which it may happen to be mixed, has always been a desideratum in chemistry. Every one conversant with the analysis of minerals is aware of the difficulty of the problem, which indeed is such that, in experiments conducted on any thing like a large scale, it might hitherto be regarded as insuperable. In consequence of this, and of the importance of the inquiry, there is hardly a chemist of eminence who has not proposed some process for the purpose, but (with the exception of that which depends on the insolubility of the persuccinate of the obnoxious metal, which I have not tried, and which is too expensive to be resorted to for any but the nicer purposes of analytical research) they are all of them either inadequate to the end proposed, intolerably tedious, or limited in their application. That which I have now to propose, on the other hand, is liable to none of these objections, being *mathematically* rigorous, of general application, and possessing in the highest degree the advantages of facility, celerity, and cheapness. It is briefly this:

The solution containing iron is to be brought to the maximum of oxidation, which can be communicated to it by boiling

* From the Transactions of the Royal Society for 1821, Part II.

with

with nitric acid. It is then to be just neutralized, *while in a state of ebullition*, by carbonate of ammonia. The whole of the iron to the last atom, is precipitated, and the whole of the other metals present (which I suppose to be manganese, cerium, nickel, and cobalt) remain in solution.

The precautions necessary to ensure success in this process are few and simple. In the first place, the solution must contain no oxide of manganese or cerium above the first degree of oxidation, otherwise it will be separated with the iron. It is scarcely probable in ordinary cases that any such should be present, the protoxides only of these metals forming salts of any stability; but should they be suspected, a short ebullition with a little sugar will reduce them to the minimum. If nitric acid be now added, the iron alone is peroxidized, the other oxides remaining at the minimum*. Moreover, in performing the precipitation the metallic solution should not be too concentrated, and must be agitated the whole time, especially towards the end of the process; and when the acid reaction is so far diminished that log-wood paper is but feebly affected by it, the alkaline solution must be added cautiously, in small quantities at a time, and in a diluted state. If too much alkali be added, a drop or two of any acid will set all right again; but it should be well observed, as upon this the whole rigour of the process depends, that no inconvenience can arise from slightly surpassing the point of precise neutralization, *as the newly-precipitated carbonates of the above enumerated metals are readily soluble, to a certain extent, in the solutions in which they are formed (though perfectly neutral)*. In the cases of cobalt and cerium, this redissolution of the recent precipitate formed by carbonate of ammonia is very considerable, and a solution of either of these metals, thus impregnated with the metallic carbonate, becomes a test of the presence of peroxide of iron, of a delicacy surpassing most of the reagents used in chemistry, the minutest trace of it being instantly thrown down by them from a boiling solution, provided no marked excess of acid be present. To be certain however that we have not gone too far, it is advisable, after separating the ferruginous precipitate, to test the clear liquid, while hot, with a drop of the alkaline carbonate. If the cloud which this produces be clearly redissolved on agitation, we may be sure that only iron has been separated. If otherwise, a little acid must

* Dr. Forschammer, in a paper recently published in Thomson's Annals of Philosophy, contends that the proto-salts of manganese are absolutely void of colour. To this I can only say, that I have not succeeded in depriving the muriate of its pale rose colour by any length of ebullition with sugar or alcohol, after which, however, not a trace of deutoxide could be detected in it. I cannot help regarding the process here proposed for freeing manganese from iron as preferable to that of Dr. F.

be

be added, the liquor poured again through the filter, so as to wash the precipitate, and the neutralization performed anew.

The precipitation of iron above described seems at first sight to result from a double decomposition. Were it so, the principle of the method would be merely a difference of solubility in the carbonates of iron and the other metals, and as such would have no claim to be regarded as rigorous. Such however is not the case. The iron is not separated in the state of a carbonate, but of a sub-salt, or a simple peroxide, the whole of the carbonic acid escaping with effervescence at each addition of the alkali. The phænomenon turns on a peculiarity in the peroxide of this metal, in virtue of which it is incapable of existing in a neutral solution at the boiling temperature. If we add an alkaline, earthy, or metallic carbonate by little and little to a *cold* solution of peroxide of iron, the precipitate formed is redissolved with effervescence, readily at first, but gradually more and more slowly, till at length many hours, or even days, elapse before the liquid becomes quite clear. Meanwhile it deepens in colour till (unless much diluted) it becomes dark brown or red. If the addition of the carbonate be carried as far as possible without producing a permanent precipitate, the solution is perfectly neutral, and continues clear at a low temperature for any length of time. In this state it may be evaporated to dryness in *vacuo*, and the residue (which *does not effervesce* with acids) is still soluble in water without letting any iron fall, and so on as often as we please.

The compound thus formed is however far from permanent. It is in fact in a state of tottering equilibrium, which a very slight cause is sufficient to overset. Supposing the point of saturation to have been exactly attained, the addition of an extremely small quantity *more* of the alkaline solution is sufficient to determine the separation of the whole, or nearly the whole, metallic contents; and if the solution operated on be pretty concentrated, it fixes after a longer or shorter time into a stiff and almost solid coagulum. Again, if to the coagulum so formed, a quantity equally inappreciable of the original ferruginous solution be added, it gradually liquefies, and after some time is completely redissolved (forming no inapt representation of the celebrated imposture of St. Januarius's blood)*.

* The phænomenon described in the text appears to me to differ from ordinary precipitations and solutions, in the small proportion between the precipitant and the precipitate, the solvent and the matter dissolved. I can call to mind but one instance of so small a quantity of matter operating a chemical change on so large a mass, viz. the decomposition of oxygenated water by fibrin and other animal substances. The action seems to be propagated from particle to particle. Whether the superabundant oxide of iron be retained in solution in a state at all analogous to that of the oxygen in Thenard's experiments, might possibly deserve consideration.

A similar

A similar change is produced by an increase of temperature. If we heat a solution exactly neutralized as above described, it speedily grows turbid, deposits its ferruginous contents in abundance, *and at the same time acquires a very decided acid reaction.* The acid so developed holds in solution a portion of oxide; but if the neutralization be performed afresh *while hot*, this separates entirely, and the liquid after filtration has no more action on gallic acid, ferrocyanate, or sulphocyanate of potash, than so much distilled water *.

It is not my object in this paper to enter into any minute detail of the nature of the persalts of iron, a subject not nearly exhausted, and which want of leisure alone has prevented my entering upon, but merely to point out the practical application of this one of their properties, to an important object in analysis. The principle here developed furnishes a ready method of detecting the minutest quantities of other metals in union with iron, and therefore cannot but prove of important service in various cases where this metal constitutes the chief ingredient in the substance examined, as in meteoric iron, the various natural oxides of this metal, &c. &c. I will exemplify this in one or two instances.

36·00 grains of meteoric iron (furnished me by the kindness of Dr. Wollaston) were dissolved in dilute nitro-sulphuric acid, leaving behind a minute quantity of a brilliant black powder, which however dissolved by digestion in nitro-muriatic acid, and appeared only to contain an excess of nickel. The solutions were mixed, and being neutralized at a boiling temperature by carbonate of ammonia, and the iron separated, a green solution remained. Into this when boiling, a drop of persulphate of iron being let fall, was immediately precipitated in the state of sub-sulphate, which being separated, the solution was boiled with excess of caustic potash till all smell of ammonia disappeared. Oxide of nickel separated, which collected and strongly ignited, weighed 4·65 grains, or 12·92 on the hundred, which (taking

* It was in 1815, in the analysis of a specimen of the gold ore of Bakebanya, given me for that purpose by Dr. Clarke, that I first remarked the separation of oxide of iron from a clear neutral solution by mere elevation of temperature, and attributed it to the presence of an oxycarbonate capable of subsisting in a low temperature, but decomposed by heat. That this is not the true explanation is already shown, and I have considerable doubt of the existence of a percarbonate of iron at *any temperature*.

The most elegant mode of exhibiting the experiment is perhaps the following: Having rendered a solution of proto-sulphate of iron rigorously neutral, by agitation with carbonate of lime and filtration, dissolve in it a small quantity of chlorate of potash (a salt perfectly neutral). The solution when raised to ebullition is peroxidized, a quantity of sub-sulphate precipitates, and the supernatant liquid is found decidedly, and even strongly acid.

the atom of nickel to weigh 30, and that of oxygen 8, hydrogen being unity) gives 10·20 per cent. for the contents of the specimen analysed in metallic nickel.

100 grains of titanious iron from North America, being dissolved in muriatic acid (after the requisite ignition with potash), were treated (after separating the titanium) with excess of carbonate of lime and filtered. The excess of carbonic acid being expelled, ammonia was added, and a small quantity of a white precipitate fell, which speedily blackened in the air, and proved to be mere oxide of manganese, uncontaminated by iron, and amounting to half a grain.

Manganese has been suspected in various species of cast iron; and though Mr. Mushet's experiments go to prove that it does not usually enter in abundance, they can hardly be regarded as establishing the fact of its absence. It might not be uninteresting to resume the investigation with the aid of a mode of analysis so well adapted to experiments on a large scale, as I have no doubt that, with proper care, one part in a thousand, or even less, of manganese might be insulated from iron.

The separation of iron from uranium cannot be accomplished by the process above described, that metal possessing a property analogous to that which forms the subject of this paper. By inverting the process, however, we shall succeed even here. A mixed solution of iron and uranium being deoxidized by a current of sulphuretted hydrogen, and then treated with an earthy carbonate, the iron passes in solution while the uranium separates. This difference in the habitudes of the two oxides of iron presents us in fact with a kind of chemical dilemma, of one or the other of whose horns we may avail ourselves in any proposed case. In studying the habitudes of uranium, however, I have met with some anomalies which require further investigation. Zirconia too might probably be freed from iron with equal facility by a similar inversion of the process; but this I have not yet had an opportunity of trying satisfactorily.

London, April 4, 1821. J. F. W. Herschel.

XIX. *Calculation of the horizontal Refraction in an Atmosphere of uniform Temperature.* By James Ivory, *M.A. F.R.S.*

It may not be ungratifying to some of the readers of the Philosophical Magazine, to have laid before them the method usually employed for computing the horizontal refraction in an atmosphere of uniform temperature, of which so much has lately been said. For this purpose, turn to the Magazine for September last,

last, and r being the horizontal refraction sought, if we make $A=0$, $\cos A=1$, $\sin A=0$, in the second of the equations (2), p. 165, we shall get

$$r=\beta\times\int\frac{dw}{\sqrt{2is-2\beta w}}.$$

In the hypothesis of a uniform temperature, the densities are proportional to the pressures, and we have $\frac{y}{y'}=1-w$: wherefore the first of the equations (2) will become $1-w=\int-ds\,(1-w)$; whence $ds=\frac{dw}{1-w}$; and $s=l\frac{1}{1-w}$. As the integral extends from $w=0$ to $w=1$, if we put $u=1-w$, and $\lambda=\frac{\beta}{i}$; we shall have $s=l\frac{1}{u}$, and

$$r=\frac{\beta}{\sqrt{2i}}\times\int\frac{du}{\sqrt{l\frac{1}{u}-\lambda+\lambda u}},$$

the integral being taken between the limits $u=0$ and $u=1$. In order to accomplish the integration, I assume

$$l\frac{1}{x}=l\frac{1}{u}-\lambda+\lambda u;$$

or,

$$lx=lu+\lambda-\lambda u;$$

and, by taking the numbers corresponding to the logarithms, we get

$$c^{-\lambda}x=uc^{-\lambda u},$$

c being the number whose hyp. log. is unit.

Let $p=\lambda c^{-\lambda}\times x$, $q=\lambda u$; then

$$p=qc^{-q}:$$

and we must now find q in a series of the powers of p. This may be effected by expanding the exponential quantity and then reverting the series; or by other well known methods by means of which the law of the terms may be discovered. I have found

$$q=p+p^2+\frac{3}{1\cdot2}p^3+\frac{4^2}{1.2.3}p^4+\&c.$$

the general term being, $\frac{n^{n-2}}{1.2.3....n-1}\times p^n$. The truth of this formula will be proved by substituting qc^{-q} for p, and then expanding all the exponentials. For it will appear that every power of q is multiplied into a coefficient of this form,

$$n^m+m.(n-1)^m+m.\frac{m-1}{2}(n-2)^m+\&c.$$

m being less than n; an expression which is known to be evanescent.

scent. The formula will therefore be reduced to the identical equation $q = q$.

By substituting the values of p and q, and then dividing by λ, we get

$$u = c^{-\lambda} \cdot x + \lambda c^{-2\lambda} \cdot x^2 + \frac{3\lambda^2 c^{-3\lambda}}{1.2} . x^3 + \&c.;$$

wherefore

$$du = dx \times \left\{ c^{-\lambda} + \frac{2\lambda c^{-2\lambda}}{1} . x + \frac{3^2\lambda^2 c^{-3\lambda}}{1.2} . x^2 + \&c. \right\},$$

and hence

$$r = \frac{\beta}{\sqrt{2i}} \times \int \frac{dx}{\sqrt{l\frac{1}{x}}} \times \left\{ c^{-\lambda} + \frac{2\lambda c^{-2\lambda}}{1} . x + \frac{3^2\lambda^2 c^{-3\lambda}}{1.2} . x^2 + \&c. \right\}.$$

All the terms of this expression are now integrable. For if we put $x^{m+1} = t$, then $l\frac{1}{x} = \frac{1}{m+1} . l\frac{1}{t}$; and, taking the integrals between $x = 0$, $t = 0$ and $x = 1$, $t = 1$, we have

$$\int \frac{x^m \, dx}{\sqrt{l\frac{1}{x}}} = \frac{1}{\sqrt{m+1}} \cdot \int \frac{dt}{\sqrt{l\frac{1}{t}}} = \frac{\sqrt{\pi}}{\sqrt{m+1}}$$

π denoting the semicircumference of which unit is the radius.

Thus we get

$$r = \frac{\beta\sqrt{\pi}}{\sqrt{2i}} \times \left\{ c^{-\lambda} + \frac{2}{1} . \frac{\lambda c^{-2\lambda}}{\sqrt{2}} + \frac{3^2}{1.2} . \frac{\lambda^2 c^{-3\lambda}}{\sqrt{3}} + \&c. \right\}, \quad (1)$$

and the series may be continued *ad libitum*, since the law of continuation is known.

If we expand the exponentials we shall obtain,

$$\begin{aligned} r = \frac{\beta\sqrt{\pi}}{\sqrt{2i}} . \times \Big\{ 1 &+ \lambda\left(\frac{2}{\sqrt{2}} - 1\right) \qquad (2) \\ &+ \frac{\lambda^2}{1.2}\left(\frac{3^2}{\sqrt{3}} - 2\frac{2^2}{\sqrt{2}} + 1\right) \\ &+ \frac{\lambda^3}{1.2.3}\left(\frac{4^3}{\sqrt{4}} - 3\frac{3^3}{\sqrt{3}} + 3\frac{2^3}{\sqrt{2}} - 1\right) \\ &+ \frac{\lambda^4}{1.2.3.4}\left(\frac{5^4}{\sqrt{5}} - 4\frac{4^4}{\sqrt{4}} + 6\frac{3^4}{\sqrt{3}} - 4\frac{2^4}{\sqrt{2}} + 1\right) \\ &+ \&c. \end{aligned}$$

Both these serieses were found by Kramp and Laplace*; but these geometers proceeded in an inverse order to that followed

* Ref. Ast. pp. 119 and 120; *Mécan. Céleste*, vol. iv. p. 252.

here.

here. They first found series (2), and then derived series (1) from it. But the analysis given here seems to have the advantage of greater simplicity.

The series (2) is more convenient for calculation, and likewise more convergent; and, in order to find the result given in the *Mécanique Céleste*, vol. iv. p. 257, we have only to substitute the values of i, β, λ, found at p. 167 of the Magazine for September last. After all, there are few physical problems of any difficulty, that admit of a more direct and satisfactory solution, or that can be brought to a calculation so easy and commodious.

Although the case of the horizontal refraction has alone been considered, yet the same analysis will apply to the general state of the problem: but, as this is attended with no difficulty, any further explanation will be unnecessary.

Feb. 4, 1822. JAMES IVORY.

XX. *On the Formation of Hail. By* A NAUTICAL CORRESPONDENT*.

THE absence of hail generally remarked by sailors navigating the Arctic regions, which observations during the late Polar expeditions have confirmed, seems to invalidate the commonly received theory of its formation from rain, precipitated by the upper regions of the atmosphere, being frozen on passing through a cold stratum of air in its descent. For were this the case, it would be but just to suppose, that instead of hail being unknown within the Arctic circle, it would bear nearly the same proportion to the rain there, that the hail bears to the rain in this country. And indeed, from the circumstance of the sea in those high latitudes being nearly covered with ice, we might reasonably infer, that a stratum of air sufficiently cold to congeal rain deposited by the higher strata of the atmosphere, would more frequently occur there than it does in this parallel.

But it will appear that this theory is contrary to general analogy; for, in ascending hills, we find the atmosphere gradually decrease in temperature, and it is well known that the summits of many mountains are covered with perpetual snow. Though currents of air of varied temperatures do occasionally occur as exceptions to this general rule, I cannot suppose the ordinary œconomy of the atmosphere to be so completely inverted as is gratuitously assumed to account for the formation of hail, unless the sudden influence of some powerful auxiliary be admitted, to produce a phænomenon so contrary to general observation.

* From the Gentleman's Magazine, vol. xci. p. 628.

If,

If, indeed, a middle stratum of cold air should occasionally intercept the falling rain in the Arctic circle, and convert it into hail, the common theory would appear more consistent; but as this is not the case, I am inclined to attribute its formation to electricity, which so frequently manifests its presence during hail showers, by thunder and lightning, and which, like hail, is unknown in high latitudes*.

Scarcely a year passes without injury being done to the crops in some part of Europe by hail showers, the stones of which are frequently as large as musket balls, plums, eggs, &c.; and Dr. Halley records instances of their being thirteen or fourteen inches in circumference, and weighing from five ounces to half a pound, which I think favours the idea, that instead of acquiring such a magnitude in their fall by accumulations round the nuclei formed by drops of congealed rain, they are generated by some sudden convulsion of the atmosphere; particularly as we know that a great portion of the air through which they must pass, if not of a temperature to diminish their bulk, is at least so warm as to prevent the congelation of any particles of vapour they might have the power of condensing round them in their descent. Now, as hail occurs most frequently when the presence of lightning shows the atmosphere to be overcharged with the electric fluid, and does not occur at all in those latitudes where lightning is unknown, I am induced to suppose, that electricity may have the power of causing a sudden expansion of the air, and consequently of generating intense cold; whereupon the particles of vapour contained in that part of the atmosphere will be immediately condensed, a number of these condensed particles (facilitated by the expansion of the air) will, by the force of their own attraction, combine, forming large drops of water, which being frozen by the excessive cold generated, descend by the laws of gravity, and produce the phænomenon of hail.

The appearance of the hail-stones (which seems to be the basis on which the common theory is founded) may, I think, be accounted for, by supposing that the central particles unite, and form drops of water before the expansion has reduced the atmosphere to the freezing temperature; that these drops are afterwards frozen, and constitute the icy centres, and that the less dense exterior coating is produced by the remaining particles being congealed before they are brought in contact. The size of the hail-stones may depend upon the degree of humidity and expansion of the air, the obstruction offered to the union of the

* During the late Polar expeditions, neither hail nor lightning was observed within the Arctic circle, nor was the atmosphere ever sufficiently charged with the electric fluid to effect the electrometer.

condensed

condensed particles of vapour, by the force of their own attraction, being in proportion to its density.

Under this impression, I can easily conceive (the resistance of the air being reduced by sudden expansion) that the condensed and frozen particles of vapour would be forcibly attracted to each other, and accumulate to the magnitude recorded in many of the hitherto apparently exaggerated accounts.

Deprived, by my early entrance into the Navy, of opportunities of acquiring philosophical knowledge, I feel conscious of my incapacity of determining a subject which does not admit of ocular demonstration; but I think it will be allowed, that the circumstances of hail being unknown within the Arctic circle, where the electric fluid is inactive, and occurring most frequently with us when our atmosphere is charged with it, are near approximations to proofs that it derives its origin from electricity. And to prove that the sudden expansion of air will generate hail, I shall, in conclusion, give the following extract from a description contained in "Gregory's Mechanics," of the Hungarian machine at Chemnitz, which discharges water from a mine by means of the compression and expansion of air. "There is a very surprising appearance in the working of this engine. On opening the cock Q" (communicating with a vessel containing compressed air and water) "the water and air will rush out together with prodigious violence, and the drops of water are changed into hail or lumps of ice. It is a sight usually shown to strangers, who are desired to hold their hats to receive the blasts of air: the ice comes out with such violence as frequently to pierce the hat like a pistol bullet."

Having shown that artificial hail is produced by the sudden expansion of air, it remains for philosophers to determine, whether or not the electric fluid could cause the air to expand in the manner I have suggested. In the mean time, as I find that I am not the first to entertain an idea of the electrical formation of hail (but the reviver of a rejected theory), I must offer a few remarks upon the objections made to it in "Rees's Cyclopædia," the work I have referred to for information on the subject. Though I may not have succeeded in proving the electrical formation of hail-stones, I think from the description given of them in the Cyclopædia, and the phænomena attendant on their fall, I shall be able to show the improbability of their being formed from drops of rain congealed by passing through a middle stratum of cold air, accumulating by accidental adhesions in their descent to the enormous sizes so frequently recorded. After giving a short account of the theory entertained by Beccaria, the writer of this article says, that "all electrical theories are inade-

quate to account for the phænomenon of hail; because, if it owed its origin to electricity, it would be a natural and ordinary production, and might be expected as frequently as rain; whereas, the quantity of hail is not more, on an average, than 1-100dth part the quantity of rain." However applicable this observation may be to Beccaria's theory, it is perfectly inapplicable to mine, for it might certainly be admitted, that the electric fluid occasionally generated hail by causing an expansion in the air, without inferring as a matter of course, that it could not exist without producing it. He observes, that "authentic accounts sufficiently testify the destruction occasioned by hail; that Mezeray mentions hail-stones which fell in Italy 100 lbs. in weight; and that Dr. Halley records some storms in which they were thirteen or fourteen inches in circumference, and weighed from five ounces to half a pound. However exaggerated some of these accounts may be," he says, "it is certainly true, that hail-stones attain a much greater size than drops of rain are ever known to do; but that the central part of every hail-stone originates in a drop of rain, is," he observes, "too obvious to require proof."

That the centres were originally drops of water is certainly evident, and perfectly agreeable to my theory; but the immense size which hail-stones occasonally attain, makes it, I think, improbable that they are generated by the tedious process assumed in the common theory; because, if they acquired their magnitude by accidental accumulations in their descent round the nuclei of drops of frozen rain, it could only be by the gradual adhesions of condensed particles of vapour, as hail-stones cannot, like drops of rain, combine, if their surfaces are accidentally brought in contact, a circumstance which is sufficiently proved by inspection: for, if it were so, instead of the central parts only resembling drops of frozen rain, there would be as many of these icy nuclei, as there were hail-stones combined. It is worthy of remark also, that although they are incapable of combining like drops of rain, they are nevertheless found to surpass them in size; and again, though they descend with much greater velocity than flakes of snow, and are consequently deprived of equal opportunities of increasing by adhesions in their descent, yet they are known to exceed them wonderfully in weight.

I am willing to allow that the accounts recorded by Mezeray and others may be exaggerated, but those mentioned by Dr. Halley ought to be received, without hesitation, for it is well known that sheep have been killed by contusions from hail-stones; and many of your readers may remember, that a few years back, the French journals were filled with accounts of subscriptions for

for the relief of the inhabitants of a little village, who had been entirely ruined by the destructive ravages occasioned by hail showers.

Instead of concurring with the common theory in supposing that the less dense exterior coating of the hail-stones ("resembling the surface of a vessel containing a freezing mixture") is formed by adhesions in their descent through a warmer stratum of air than that in which the nuclei were generated, I have attributed it, in my theory, to the *increase* of cold, by which the particles of vapour are frozen before they adhere to their respective nuclei, when in consequence of the attractive power, exerted upon the frozen particles of vapour by the nuclei, not being sufficient to make them cohere as closely as if in a fluid state, the exterior coating must, agreeable to observation, be of a less dense nature. Though drops of rain are liable to sudden accessions by running into each other, the influence of the electric fluid is sufficiently obvious in thunder showers, by the uniform magnitude of the drops: why its influence in hail showers, which seldom occur unaccompanied by thunder and lightning, should be doubted, I cannot conceive, for certainly there is nothing in the appearance of the stones which opposes the probability of their electrical formation, and it is the only way in which their size can be reasonably accounted for.

The circumstance of hail being usually accompanied by thunder and lightning, is not allowed in the Cyclopædia to be a proof that the superabundance of electric fluid operates in its formation, but that thunder happens when the atmosphere is most replete with vapour, which is also favourable to the generation of hail.

I have already observed in my theory, that I conceived the degree of humidity of the atmosphere would operate as one cause in regulating the size of the hail-stones; but as the electric fluid is inactive in the higher latitudes where hail is unknown, though there is no want of vapour to produce rain and snow, I think it appears evident, that "hail is the attendant on thunder," because it owes its origin to electricity.

XXI. *True apparent Right Ascension of Dr.* Maskelyne's 36 *Stars for every Day in the Year* 1822, *at the Time of passing the Meridian of Greenwich. By the Rev.* J. Grooby.

The mean Right Ascensions are taken from Mr. Pond's Catalogue in the Nautical Almanac for 1823, and the Corrections from the Tables of M. Bessel. On those days where an asterisk is prefixed the Star passes twice, the Æ there given is that at the first passage.

1822.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 52	H. M. 4 25	H. M. 5 3	H. M. 5 5	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 30	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 40	H. M. 11 41	H. M. 13 15	H. M. 16 7
May	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	5·55	9·98	59·49	43·69	34·10	59·79	3·75	33·21	18·77	15·41	0·23	26·46	52·18	55.36	1·27	28·13	52·54	35·96
2	57	00	49	69	10	78	75	20	76	40	22	44	17	34	26	12	54	96
3	59	10·01	50	69	09	78	74	19	75	39	21	43	16	33	26	12	54	96
4	62	03	51	69	09	78	74	19	74	37	19	42	15	32	25	11	54	97
5	64	04	52	69	08	77	73	18	73	36	18	40	13	31	24	10	54	97
6	66	06	* 53	69	08	77	73	18	72	35	17	39	12	30	23	10	54	98
7	68	07	55	69	08	77	73	17	71	34	16	38	11	29	23	09	54	98
8	71	09	56	69	07	76	72	17	69	32	15	36	09	27	22	08	54	98
9	74	11	57	69	07	76	72	16	68	31	14	35	08	26	21	08	54	99
10	77	13	58	70	07	76	72	16	67	29	13	33	07	25	20	07	54	99
11	79	15	59	70	07	76	72	16	66	28	12	32	05	24	19	06	54	99
12	82	17	60	70	07	75	72	15	65	27	11	31	04	23	19	05	53	99
13	85	19	61	71	07	75	72	15	64	26	10	30	03	21	18	05	53	99
14	87	21	63	71	07	75	72	15	64	25	09	29	02	20	17	04	53	36·00
15	90	23	64	72	07	75	72	14	63	24	08	28	01	19	16	03	53	00
16	92	25	65	72	07	75	73	14	62	23	07	27	51·99	18	15	02	53	00
17	95	27	66	73	07	75	73	14	61	22	06	27	98	16	14	02	52	00
18	98	29	68	73	08	75	73	14	61	21	05	26	97	15	13	01	52	00
19	6·01	31	70	74	08	75	73	14	60	21	05	25	96	14	12	00	52	00
20	04	33	72	75	09	75	74	14	59	20	04	24	95	13	11	27·99	52	00
21	97	36	73	76	09	76	74	14	59	19	03	24	94	12	10	98	51	00
22	10	38	75	77	10	76	75	14	58	18	02	23	93	11	09	98	51	00
23	12	40	77	78	10	76	75	15	58	18	02	22	92	10	08	97	51	00
24	15	43	79	79	11	77	76	15	57	17	01	21	91	09	07	96	51	35·99
25	18	45	80	80	12	77	76	15	57	16	01	21	90	08	06	95	50	99
26	21	47	82	81	12	77	77	15	56	16	00	20	89	06	05	94	50	99
27	24	50	84	82	13	78	77	15	56	15	00	19	88	05	04	93	50	99
28	27	52	85	83	14	78	78	16	56	15	29·59·99	19	87	04	03	92	49	98
29	30	55	87	* 84	15	79	78	16	55	14	99	18	85	03	02	91	49	98
30	33	57	89	86	16	79	79	16	55	14	99	18	84	02	01	91	48	98
31	36	60	92	87	18	80	80	17	55	14	98	18	83	01	00	90	48	98

1822.	1α Libræ.	2α Libræ.	α Cor. Bor.	α Serpentis.	Antares.	α Herculis.	α Ophiuchi.	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1α Capri.	2α Capri.	α Cygni.	α Aqua.	Fomalhaut	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	M. H. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
May	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	54·59	6·03	12·35	33.44	33·75	34·90	43·32	56·95	49 92	7.94	36.25	48·66	12·41	23·08	39·76	49·00	55·01	12·86
2	60	04	36	45	77	92	34	98	95	97	28	69	44	12	79	03	04	89
3	62	06	37	47	79	95	37	57·00	98	8·00	31	72	47	15	82	06	07	91
4	63	07	39	48	81	97	39	03	50·01	03	34	75	50	19	85	09	10	93
5	64	08	40	49	83	99	41	06	04	06	37	79	54	23	88	12	13	96
6	65	09	41	51	85	01	43	09	07	09	40	82	57	27	91	15	15	99
7	66	10	42	52	87	03	45	12	10	12	43	85	60	30	94	18	18	13·01
8	67	11	43	53	89	05	48	15	13	15	46	88	63	34	96	21	21	04
9	68	12	44	55	91	07	50	18	15	18	49	91	66	38	40·00	25	23	07
10	69	13	45	56	93	09	52	21	19	21	52	94	69	42	03	28	27	10
11	70	14	46	57	95	11	54	24	22	24	55	97	72	45	06	31	30	13
12	70	14	46	58	96	12	56	26	24	27	58	49·00	75	49	09	35	33	15
13	71	15	47	59	98	14	58	29	27	30	61	04	79	52	12	38	36	18
14	72	16	48	60	34·00	16	60	31	30	33	64	07	82	56	15	41	39	21
15	72	16	49	61	02	18	62	34	33	36	67	10	85	60	18	45	42	24
16	73	17	50	62	03	19	64	37	35	39	70	13	88	63	21	48	45	27
17	74	18	50	63	05	21	66	39	38	42	73	16	91	67	24	51	48	30
18	74	18	51	64	07	23	68	42	41	45	76	19	94	70	27	55	51	33
19	75	19	52	65	09	25	70	45	44	47	78	23	98	74	31	58	54	36
20	75	19	53	66	10	26	72	47	47	50	81	26	13·01	78	34	62	57	39
21	76	20	53	67	11	28	74	49	50	52	84	29	04	81	37	65	60	42
22	76	20	54	67	13	29	75	52	53	55	86	32	07	85	40	69	63	45
23	76	20	54	68	14	31	77	54	55	58	89	35	10	88	43	72	67	48
24	77	21	55	69	15	32	79	56	57	61	92	38	13	91	47	76	70	52
25	77	21	55	69	17	33	80	58	60	63	94	41	16	95	50	79	73	55
26	77	21	56	70	18	35	82	61	63	66	97	44	19	98	53	83	76	58
27	78	22	56	71	19	36	84	63	65	68	37·00	47	22	24·02	56	86	79	61
28	78	22	57	71	20	37	85	65	68	71	02	50	25	05	59	89	82	64
29	78	22	57	72	22	39	87	68	71	74	05	53	28	09	63	93	86	68
30	78	22	57	72	23	40	88	70	74	77	08	55	30	12	66	97	90	72
31	79	23	57	73	24	41	90	71	76	79	10	58	33	16	69	50·00	93	75

1822.	γ Pegasi	α Arietis.	α Ceti.	Aldebaran	Capella	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 52	H. M. 4 25	H. M. 5 3	H. M. 5 5	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 30	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 40	H. M. 11 41	H. M. 13 15	H. M. 16 7
June	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	6·40	10·63	59 94	43·89	34·19	59 81	3·81	33·17	18·55	15·13	29·59 98	26·17	51·82	55·00	0·99	27·89	52·47	35·97
2	43	65	96	91	20	82	82	18	55	13	98	17	81	54·99	98	88	47	97
3	46	68	98	92	22	82	83	18	54	13	97	16	80	98	97	87	46	96
4	49	71	53·0·00	93	23	83	84	19	54	13	97	16	79	97	96	86	45	96
5	52	73	02	95	24	84	85	20	54	13	97	16	78	96	95	85	45	95
6	55	76	04	96	25	85	86	20	54	12	97	16	77	95	93	84	44	94
7	58	79	06	97	27	86	87	21	54	12	97	16	76	94	92	83	44	94
8	61	82	08	99	30	* 87	89	22	54	12	96	15	76	93	91	82	43	93
9	65	85	11	44·01	31	89	90	22	54	12	96	15	75	92	90	82	42	92
10	68	88	13	03	33	90	* 92	23	54	12	96	15	74	91	89	81	41	92
11	71	91	16	05	35	91	95	24	55	12	96	15	73	91	88	80	41	91
12	74	94	18	06	37	92	96	25	55	12	96	15	72	90	87	79	40	90
13	77	97	20	08	38	93	97	26	55	12	96	15	72	89	86	78	39	90
14	81	00	23	10	40	94	99	27	55	12	96	15	71	88	85	77	39	89
15	84	11·03	25	12	42	96	4·00	28	56	13	96	15	70	88	84	76	38	88
16	87	06	27	13	43	97	02	29	56	13	96	15	70	87	83	75	37	88
17	90	09	30	15	45	99	03	30	56	13	96	15	69	86	82	74	37	87
18	93	12	32	17	48	6·0·01	05	* 32	57	13	96	15	69	85	81	73	36	86
19	96	15	35	19	50	02	07	35	58	14	96	16	68	84	80	73	35	85
20	7·00	19	38	22	53	04	09	36	59	14	97	16	68	84	79	72	34	85
21	03	22	41	24	56	05	11	37	59	15	97	16	67	83	78	71	34	84
22	06	25	43	26	58	07	12	39	60	15	97	17	67	82	77	70	33	83
23	09	28	46	28	61	09	14	40	61	16	98	17	66	82	76	69	32	82
24	12	31	49	30	63	10	16	41	61	16	98	18	66	81	75	69	31	81
25	16	34	51	32	66	12	18	42	62	17	99	18	65	81	74	68	31	80
26	19	38	54	35	68	13	20	44	63	18	99	19	65	80	73	67	30	79
27	22	41	57	37	71	15	22	45	63	18	30·0·00	20	64	80	72	66	29	78
28	25	44	60	40	73	17	25	47	64	19	00	20	64	79	71	65	28	77
29	28	48	62	42	76	19	27	48	65	20	01	21	64	79	70	64	27	76
30	32	51	65	45	79	21	29	50	* 66	21	01	22	63	78	69	63	26	75

1822.	1 α Libræ.	2 α Libræ.	α Cor. Bor.	α Serpentis.	Antares.	α Herculis.	α Ophiuchi.	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1 α Capri.	2 α Capri.	α Cygni.	α Aqua.	Fomalhaut.	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
June	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	54·79	6·23	12·57	33·73	34·26	34·42	43·91	57·73	50 79	8 82	37 13	49 61	13·36	24·19	40·72	50 04	55·96	13·78
2	79	23	57	74	27	43	92	75	81	84	15	61	39	22	75	07	99	82
3	79	23	57	74	28	45	93	77	83	87	18	66	41	25	79	11	56·03	85
4	79	23	57	74	29	46	95	79	86	89	20	69	44	29	82	15	06	89
5	79	23	57	75	30	47	96	81	88	92	23	72	47	32	85	18	09	92
6	79	23	57	75	31	48	97	83	90	94	25	75	50	35	88	21	12	95
7	79	23	57	75	32	49	98	85	93	97	28	78	53	38	91	25	15	98
8	79	23	57	76	33	50	44·00	87	96	99	31	81	56	42	94	29	19	14 02
9	79	23	57	76	34	51	·01	88	98	9 01	33	83	58	45	97	33	21	05
10	79	23	56	76	35	52	02	90	51·00	04	35	86	61	48	41·00	36	24	09
11	78	22	56	76	35	53	03	91	02	06	38	88	63	50	03	40	27	12
12	78	22	56	77	36	53	04	93	04	08	40	91	66	53	06	43	31	16
13	78	22	56	77	37	54	05	94	06	10	42	93	68	56	09	46	34	19
14	78	22	56	77	37	55	06	96	08	12	44	96	71	59	12	50	37	23
15	78	22	55	77	38	56	07	97	11	15	46	98	73	62	15	53	40	26
16	77	21	55	77	38	57	08	98	13	17	49	50 01	76	64	18	57	43	30
17	77	21	55	77	39	58	09	58·00	15	18	51	03	78	67	21	60	46	33
18	77	21	55	77	40	58	10	01	17	21	53	06	81	70	24	64	50	37
19	76	20	54	77	40	59	11	02	19	23	55	09	84	72	27	68	53	40
20	76	20	54	76	40	60	11	03	21	25	57	11	86	75	30	71	56	44
21	76	20	54	76	41	60	12	04	23	27	59	13	88	77	33	74	59	47
22	75	19	53	76	41	61	13	05	24	29	61	15	90	80	36	78	62	51
23	75	19	53	76	41	61	14	06	26	31	63	18	92	82	39	81	65	54
24	74	18	52	76	41	62	14	07	28	33	65	20	95	85	41	85	69	58
25	74	18	51	76	42	62	15	08	30	34	67	22	97	87	44	88	72	61
26	73	17	51	75	42	63	16	09	32	36	69	24	99	90	47	91	75	64
27	72	16	50	75	42	63	16	10	34	38	71	27	14·02	92	50	95	78	68
28	72	16	49	75	42	64	17	11	36	40	72	29	04	95	53	98	81	71
29	71	15	48	75	42	64	17	11	38	41	74	31	06	97	55	51·01	84	74
30	70	14	47	74	42	64	17	12	[illegible]	43	76	33	08	99	58	04	87	78

XXII. *On the Circle, the Sphere, the Square, and the equilateral Triangle.* By Mr. JAMES UTTING.

To Dr. Tilloch.

DEAR SIR, —SHOULD the following statement relative to the sides and areas of the ⊙, □, and equilateral △, &c. be thought worthy a place in the Philosophical Magazine, it is at your service.

Circumference of a ⊙ to diameter unity	3·14159,26535,89793,23846
Area of a ⊙ to diameter unity	·78539,81633,97448,30962
Solidity of a sphere to diameter unity	·52359,87755,98298,87308
The diameter of a ⊙ = 1.	
The side of the circumscribed equilateral △	= 1·73205,08075,68877,29353
The diameter of a ⊙ = 1.	
The side of the inscribed equilateral △ ..	= ·86602,54037,84438,64676
The diameter of a ⊙ = 1.	
The side of an equilateral △ of equal area	= 1·34677,36870,886
The diameter of a ⊙ = 1.	
The side of a □ of the same area	= ·88622,69254,52758,01365
The diameter of a ⊙ = 1.	
The side of the inscribed □	= ·70710,67811,86547,52440
The side of a □ = 1.	
The diameter of the circumscribed ⊙ ..	= 1·41421,35623,73095,04880
The side of a □ = 1.	
The side of the circumscribed △	= 2·15470,05383,79251,52902
The side of a □ = 1.	
The side of an equilateral △ of the same area	= 1·51967,13713,03185,09466
The side of an △ = 1.	
The diameter of its inscribed ⊙	= ·57735,02691,89625,76451
The side of an △ = 1.	
The diameter of its circumscribed ⊙ ..	= 1·15470,05383,79251,52902
The side of an △ = 1.	
The diameter of a ⊙ of equal area	= ·74251,52492,857

The

The side of an △ = 1.

The side of the inscribed □ .. .. } = ·46530,24295,51049,79947

The side of an △ = 1.

The side of a □ of equal area .. .. } = ·65803,70064,76246,23041

The side of an △ given to find its area × the □ of the side by } 0·43301,27018,92219,32338

The lines circumscribing a □, and ⊙, of the same area, are in proportion to each other as 3·54490,77018,11032,05460, to 3·14159, &c. Or, as 1. to ·88622,69254,52758,01365.

The lines circumscribing a □, and △, of the same area, are as 1. to 1·51967,13713,03185,09466.

The lines circumscribing an △, and ⊙, of the same area, are as 1. to ·77756,01507,781066.

The area of a hexagon to that of its circumscribing circle is as 1. to 1·20919,9576156. Or, as ·82699,33431,32688 to 1.

Lynn Regis, Nov. 15, 1821. JAMES UTTING.

ERRATA.—In the Table of the ⊙'s R.A. in degrees, &c. vol. lvii. page 29:

Argument.		R.A.	Diff.		R.A.	Diff.
s. ° ′						
1 3 20	*for*	31° 6′ 11″·53	577·72 / 578·39	*read*	31° 6′ 11″·73	577″·92 / 578·19

Argument.		Diff.		
s. ° ′				
1 14 20 / 1 14 30 / 1 14 40	*for*	597″·76 / 597·98	*read*	596″·76 / 596·98

Argument.		R.A.		
s. ° ′				
1 16 50	*for*	44° 21′ 41″·98	*read*	44° 21′ 41″·98

In the Table of the ⊙'s R.A. in time, page 184:

Argument.		Diff.		
s. ° ′				
0 13 0 / 0 13 10	*for*	36″·00	*read*	37″·00

In the Reduction of the Ecliptic to the Equator, page 435:

Argument.		Reduction.		
s. ° ′				
2 3 30	*for*	2° 1′ 31″·97	*read*	2° 1′ 31″·96

The signs, &c. at the bottom of pages 435 and 436 are wrong inserted, they ought to be the same as at the bottom of pages 437 and 438.

In the Table of the Var. of the ⊙'s R.A. and Decl., page 440:

Argument.		Var. R.A.		
s. ° ′				
0 7 50	*for*	5″·49	*read*	5″·39
		Var. Declin.		
1 10 50	*for*	61·13	*read*	62·13

XXIII. *Letter from* Robert Hare, *M.D. Professor of Chemistry in the University of Pennsylvania, &c. &c. to the Editor of the American Journal of Science and Arts, in Opposition to the Conjecture that Heat may be Motion, and in favour of the Existence of a material Cause of calorific Repulsion* *.

Dear Sir,—In two memoirs published in your Journal, I have endeavoured to show that caloric and electricity are collateral agents in galvanism, the ratio of the former to the latter, in quantity, being as the extent of the operating superficies to the number of pairs into which it may be divided. In those publications, I assumed that the causes of heat and electricity are material fluids. Although this view of the origin of calorific repulsion is taken by a great majority of chemists, it has been combated, both by Rumford and Davy: the former famous for his ingenious, instructive and laborious experiments; and the latter distinguished by the most splendid discoveries. With the utmost deference for the authority of these great men, especially the latter, I send the following remarks made in answer to his hypothetical views, which I shall here quote from his Elements in order to introduce the subject more intelligibly.

"It seems possible," says the illustrious author, "to account for all the phænomena of heat, if it be supposed, that in solids the particles are in a constant state of vibratory motion, the particles of the hottest bodies moving with the greatest velocity, and through the greatest space; that in liquids and elastic fluids, besides the vibratory motion, which must be conceived greatest in the last, the particles have a motion round their own axes, with different velocities, the particles of elastic fluids moving with the greatest quickness; and that in ethereal substances, the particles move round their own axes, and, separating from each other, penetrate in right lines through space. Temperature may be conceived to depend upon the velocities of the vibrations; increase of capacity on the motion being performed in greater space; and the diminution of temperature, during the conversion of solids into fluids or gases, may be explained on the idea of the loss of vibratory motion, in consequence of the revolution of particles round their axes, at the moment when the body becomes liquid or aëriform; or from the loss of rapidity of vibration, in consequence of the motion of the particles through greater space.

"If a specific fluid of heat be admitted, it must be supposed liable to most of the affections which the particles of common matter are assumed to possess, to account for the phænomena;

* From Silliman's Journal, No. IX.

such

such as losing its motion when combining with bodies, producing motion when transmitted from one body to another, and gaining projectile motion when passing into free space; so that many hypotheses must be adopted to account for its agency, which renders this view of the subject less simple than the other. Very delicate experiments have been made, which show that bodies, when heated, do not increase in weight. This, as far as it goes, is an evidence against a subtile elastic fluid, producing the calorific expansion; but it cannot be considered as decisive on account of the imperfection of our instruments. A cubical inch of inflammable air requires a good balance to ascertain that it has any sensible weight, and a substance bearing the same relation to this, that this bears to platinum, could not perhaps be weighed by any method in our possession."

These suggestions of Sir H. Davy's are to me unsatisfactory.

It is fully established in mechanics, that when a body in motion is blended with and thus made to communicate motion to another body, previously at rest, or moving slower, the velocity of the compound mass after the impact will be found, by multiplying the weight of each body by its respective velocity, and dividing the sum of the products by the aggregate weight of both bodies. Of course it will be more than a mean or less than a mean, accordingly as the quicker body was lighter or heavier than the other. Now, according to Sir Humphry Davy, the particles of substances which are unequally heated are moving with unequal degrees of velocity: of course when they are reduced by contact to a common temperature, the heat, or, what is the same (in his view), the velocity of the movements of their particles, ought to be found by multiplying the heat of each by its weight and dividing the sum of the product by the aggregate weight. Hence if equal weights of matter be mixed, the temperature ought to be a mean; and if equal bulks, it ought to be as much nearer the previous temperature of the heavier substance as the weight of the latter is greater; but the opposite is in most instances true. When equiponderant quantities of mercury and water are mixed at different temperatures, the result is such as might be expected from the mixture of the water, were it twenty-six times heavier; so much nearer to the previous heat of the water is the consequent temperature. It may be said that this motion is not measurable upon mechanical principles. How then, I ask, does it produce mechanical effects? These must be produced by the force of the vibrations, which are by the hypothesis mechanical: for whatever laws hold good in relation to moving matter in mass, must operate in regard to each particle of that matter; the effect of the former can only be a multiple of that of the latter. Indeed, one of Sir Humphry Davy's reasons for thinking heat to

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consist of corpuscular motions is, that mechanical attrition generates it. Surely then a motion produced by mechanical means, and which produces mechanical effects, may be estimated on mechanical principles.

In the case cited above, the power of reciprocal communication of heat in two fluids, is shown to be inconsistent with the views of this ingenious theorist. If we compare the same power in solids, the result will be equally objectionable. Thus the heating power of glass being 443, that of an equal bulk of lead will be 487, though so many times heavier; and if equal weights be compared, the effect of the glass will be four times greater than that of the lead. If it be said, that the movements of the denser matter are made in less space, and therefore require less motion, I answer, that if they be made with equal velocity, they must go through equal space in the same time, their alternations being more frequent. And if they be not made with the same velocity, they could not communicate to matter of a lighter kind a heat equally great; since, agreeably to experience, no superiority of weight will enable a body, acting directly on another, to produce in it a motion quicker than its own. Consistently with this doctrine, the particles of an aëriform fluid, when they oppose a mechanical resistance, do it by aid of a certain movement, which causes them effectively to occupy a greater space than when at rest. It is true, a body by moving backwards and forwards may keep off other bodies from the space in which it moves. Thus, let a weight be partially counterbalanced by means of a scale beam, so that if left to itself it would descend gently. Place exactly under it another equally solid mass, on which the weight would fall. If between the two bodies thus situated a third be caused to undergo an alternate motion, it may keep the upper weight from descending, provided the force with which the latter descends be no greater than that of the movement in the interposed mass, and the latter acts with such celerity, that between each stroke the time be too small for the weight to move any sensible distance. Here then we have a case analogous to that supposed, in which the alternate movements or vibrations of matter enable it to preserve to itself a greater space in opposition to a force impressed; and it must be evident that lengthening or shortening the extent of the vibrations of the interposed body, provided they are made in the same time, will increase or diminish the space apparently occupied by it, as the volume of substances is affected by an increase or reduction of heat. It ought however to be recollected that in the case we have imagined, there is a constant expenditure of momentum to compensate for that generated in the weight by gravity, during each vibration. In the vibrations conceived to constitute heat, there is no gene-

rating

rating power to make up for this loss. A body preserves the expansion communicated by heat *in vacuo*, where, insulated from all other matter, the only momentum, by which the vibrations of its particles can be supported, must have been received before its being thus situated. If we pour mercury into a glass tube shaped like a shepherd's crook, the hook being downwards, the fluid will be prevented from occupying that part of the tube where the air is in such position as not to escape. In this case, according to the hypothesis in question, the mercury is prevented from entering the space the air occupies, by a series of impalpable gyratory movements; so that the collision of the aërial particles against each other, causes each to occupy a larger share of space in the manner above illustrated by the descending weight and interposed body. The analogy will be greater, if we suppose a row of interposed bodies alternately striking against each other, and the descending weight; or we may imagine a vibration in all the particles of the interposed mass equal in aggregate extent and force to that of the whole, when performing a common movement. If the aggregate extent of the vibration of the particles very much exceed that which when performed in mass would be necessary to preserve a certain space, it may be supposed productive of a substance like the air by which the mercury is resisted. But whence is the momentum adequate in such rare media to resist a pressure of a fluid so heavy as mercury, which in this case performs a part similar to that of the weight, cited for the purpose of illustration? If it be said that the mercury and glass being at the same temperature as the air, the particles of these substances vibrate in a manner to keep up the aërial pulsations; I ask, when the experiment is tried in an exhausted receiver, what is to supply momentum to the mercury and glass? There is no small difficulty in conceiving under the most favourable circumstances, that a species of motion, that exists according to the hypothesis as the cause of expansion in a heated solid, should cause a motion productive of fluidity or vaporization, as when by means of a hot iron we convert ice into water, and water into vapour.

How inconceivable is it that the iron boiler of a steam engine should give to the particles of water, a motion so totally different from any it can itself possess, and at the same time capable of such wonderful effects, as are produced by the agency of steam! Is it to be imagined that in particles whose weight does not exceed a few ounces, sufficient momentum can be accumulated to move as many tons? There appears to me another very serious obstacle to this explanation of the nature of heat. How are we to account for its radiation *in vacuo*, which the distinguished advocate of the hypothesis has himself shown to ensue? There

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can be no motion without matter. To surmount this difficulty, he calls up a suggestion of Newton's, that the calorific vibrations of matter may send off radiant particles, which lose their own momentum in communicating vibrations to bodies remote from those whence they emanate. Thus, according to Sir Humphry, there is radiant matter producing heat, and radiant matter producing light. Now, the only serious objection made by him to the doctrine which considers heat as material, will apply equally against the existence of material calorific emanations. That the cannon, heated by friction in the noted experiment of Rumford, would have radiated as well as if heated in any other way, there can, I think, be no doubt; and as well *in vacuo*, as the heat excited by Sir Humphry in a similar situation. That its emission in this way would have been as inexhaustible as by the conducting process cannot be questioned. Why then is it not as easy to have an inexhaustible supply of heat as a material substance, as to have an inexhaustible supply of radiant matter, communicating the vibrations in which he represents heat to consist?

We see the same matter, at different times, rendered self-attractive, or self-repellent; now cohering in the solid form with great tenacity, and now flying apart with explosive violence in the state of vapour. Hence the existence, in nature, of two opposite kinds of reaction, between particles, is self-evident. There can be no property without matter, in which it may be inherent. Nothing can have no property. The question then is, whether these opposite properties can belong to the same particles. Is it not evident, that the same particles cannot, at the same time, be self-repellent, and self-attractive? Suppose them to be so, one of the two properties must predominate, and in that case we should not perceive the existence of the other. It would be useless, and the particles would in effect possess the predominant property alone, whether attraction or repulsion. If the properties were equal in power, they would annihilate each other, and the matter would be, as if void of either property. There must, therefore, be a matter, in which the self-repellent power resides, as well as matter in which attraction resides.

There must also be as many kinds of matter, as there are kinds of repulsion, of which the affinities, means of production, or laws of communication, are different. Hence I do firmly believe in the existence of material fluids, severally producing the phænomena of heat, light, and electricity. Substances, endowed with attraction, make themselves known to us, by that species of this power, which we call gravitation, by which they are drawn towards the earth, and are therefore heavy and ponderable; by their resistance to our bodies, producing the sensation of feeling or touch; and by the vibrations or movements in other matter,

affecting

affecting the ear with sounds, and the eye by a modified reflection of light. Where we perceive none of these usual concomitants of matter, we are prone to infer its absence. Hence ignorant people have no idea of air, except in the state of wind; and when even in a quiescent state designate it by this word. But that the principles, the existence of which has been demonstrated, should not be thus perceived, is far from being a reason for doubting their existence. A very slight attention to their qualities will make it evident, that they could not produce any of the effects, by which the existence of matter in its ordinary form is recognised. The self-repellent property renders it impossible that they should resist penetration; their deficiency of weight renders their movements nugatory. When in combination, *they* are not perceived, but the *bodies* with which they combine; and it is only by the changes they produce in such bodies, or their effects upon our nerves, that they can be detected.

XXIV. *On the Breeding of Eels.* *By* ANTHONY CARLISLE, *Esq.*

To Dr. Tilloch.

DEAR SIR,—SOME years ago I suggested to several naturalists the probability that the common river eel effected its procreation exclusively in the sea; and as my professional engagements now supersede such pursuits, I submit my reasons for entertaining that opinion to persons who may have opportunities of ascertaining the necessary proofs. It is notorious that the common eel is never taken in fresh water with either the male or female organs distinctly pronounced. It is also known that those fishes descend rivers toward the sea; and at those seasons they are caught abundantly in wears and by other contrivances: but those grown eels never return again up the rivers or streams, and therefore either finally perish in the ocean or remain permanently for some special occasion. At particular periods small eels ascend rivers in vast shoals, and toward the mouths of rivers they are found of smaller dimensions, and gradually attain growth as they advance up the streams: for example, the small grigs caught so copiously in wicker baskets, and in Chinese dipnets, or by bobbing with threaded worms in the river Thames, are never found of the same small sizes toward Oxford, as they appear within the tiding range of the river. About three years since, when dissecting a conger eel, I discovered a matured female roe, the ova being ripe for detachment from the parent membranes; and on comparing this animal with the common eel, I could not discern any distinctive difference, either external or internal, beyond those trivial deviations which occur to the species of many other creatures.

tures. On further inquiry, I found the conger eel to be a regular breeding fish with special sexual organs. It is therefore more than probable that the conger is the breeding eel, and that it never returns into fresh water after its entrance into the ocean. This peculiarity is different from the salmon, which alternates its periodical visits between the rivers and sea for breeding purposes: but Nature observes general, and not universal rules. When at Hastings several years ago, I put small eels caught from a neighbouring brook into sea-water, and they enjoyed apparent vigour for many days successively. I have also, when a boy, frequently caught river eels on the salt-water side of a marsh sluice at the mouth of the river Tees.

Any person resident on the coast might easily determine the leading facts respecting the identity, or otherwise, of conger and river eels. I suspect the latter require some years of sea growth before they acquire the sexual parts, but no degree of fresh water growth ever develops those organs in a river eel.

Dear sir,

Your obliged servant,

3, Langham Place, Cavendish Square, Jan. 12, 1822. ANTHONY CARLISLE.

XXV. *On the Use of Phosphoric Acid in Jaundice. By Dr.* CALEB MILLER*.

To Prof. Silliman.

Bristol, (R. I.) April 28, 1821.

DEAR SIR,—SEEING in your Journal that you solicit communications, for the promotion of the Arts and Sciences, from the effects I have seen produced from the phosphoric acid in the cure of the jaundice, I am induced to say something of what I know, as I have not seen any mention of this acid as a remedy in that disease.

About six years ago I had a very obstinate case that resisted the common remedies. I was led to use the phosphoric acid on the principle that the acids decompose the bile. I made choice of this on account of its existing in a separate state in the blood.

I directed a large spoonful of the acid as prepared in Murray's Materia Medica in a pint of balm tea to be taken as fast as the stomach would bear it, till it should operate as a diuretic. In twenty-four hours the patient had taken eight pints, and it had operated powerfully as a diuretic. Neither the urine nor the white of the eye was as yellow as before, by a very obvious dif-

* From Silliman's Journal, No. IX.

ference.

ference. I ordered a continuance under the same directions, and in two days more the urine was of nearly the natural colour; but the skin had not improved in the same proportion. I advised tonics with the occasional use of the acid, and my patient shortly recovered.

I have had many of the same complaint since that time, and have directed nearly in the manner, according to the age and condition of the patients, and the result has been the restoration of health in a very short time. In general, the yellowness disappeared in three or four days from the urine, but continued a little longer on the skin; by the use of tonics, and sometimes a little of the acid, this is however removed in a few days. I have met with only one patient, whose symptoms have not yielded to the above plan. This was a person eighty years of age. Even in this case, however, the acid always produced relief; but the complaint soon returned. My present practice is to give a cathartic of calomel and julep or some of the neutral salts, and then the balm tea moderately acidulated with the phosphoric acid, which I direct to have continued till it operates as a diuretic and until the urine becomes clear or nearly so; this commonly takes place in the course of two days. I have advised other acids when this has not been at hand; but I am inclined to give the preference to the phosphoric, although I think the others deserve a further trial.

I might have entered much more into detail, but I am satisfied that it needs only a trial to convince any candid person of the advantage of this acid in the cure of the jaundice. I have never seen any bad effects from the use of the phosphoric acid, although it is said that phosphorus is poisonous. This I have never used.

I shall be happy to answer any inquiries, and remain respectfully your obedient servant,

Caleb Miller.

XXVI. *On the Culture of Indian Corn, &c.* By John Murray, *F.L.S. M.W.S., &c. &c.*

To Dr. Tilloch.

London, Feb. 7, 1822.

Sir, — In No. 284, page 433, of the "Philosophical Magazine and Journal," we are favoured with "Thoughts on the Cultivation of Maize, &c. by a practical and experimental Farmer." It is indeed an inquiry of considerable interest. That Indian corn has ripened in this country, and that too without artificial warmth or shelter, is a well ascertained fact, and such seeds would doubtless ensure a succession more hardy than the primitive seed, whence

whence the successional crops were derived. An intelligent practical farmer in Holderness informed me he had succeeded in rearing Indian corn in an unsheltered and exposed situation, and that the seed thus obtained grew freely. In the North of Italy, the growers of maize *twitch off the tops* of the plants as soon as the male flowers have done their part, and this is allowed to accelerate the expansion and ripening of the seeds.

In the Neapolitan kingdom they possess a species or variety of the Indian corn, called *Mellica quarantina*, which is sown as a successional crop after the wheats are reaped. It is presumed ripe for the sickle in forty days, and from this circumstance receives its specific distinction. This variety of maize seems admirably adapted for the short season of our summer. I can have no doubt whatever of its capability of being naturalized to this climate. By some preparative, as steeping the corn in water of a genial warmth, ere it is committed to the earth, we might determine promptly the germinative powers, and anticipate in some measure the period requisite for its full and perfect development. Mr. Knight has with his usual acuteness and sagacity insisted on the importance (in cases of exotic plants) of an instant stimulus of increased temperature, and he very judiciously appeals to what can be accomplished by a short Canadian summer, to prove the correctness of his views —I take it that we suffer most in the *sudden transit* from the chills of night to the blaze of sunshine, and that if practicable we should screen our wall trees, &c. by brushwood (and it is astonishing how small a matter will accomplish the purpose in question), not only as a defence from the loss of temperature sustained by radiation in a climate, where the period, at night, in relation to the thermometer *above the freezing* point, forms so small a fraction of the year. I am glad to see so much science enlisted into the service of argiculture and horticulture as we find in the persons of Sir Humphry Davy, Mr. Knight, &c. and it augurs well for their further advancement. The nutritious properties of Indian corn remain undoubted.

Permit me to add, that I think it singular *Millet* should not be attempted as a green crop for cattle, if not for its seeds. It is very generally cultivated in Tuscany, and cut down, while still unripe, as food for cattle. I have reared it for my amusement, and thus know it to be equally hardy with Canary-grass; consequently may be cultivated for the purpose in question. In the Southern counties of England, I doubt not it might profitably succeed the crops of wheat.

There is cultivated in Italy, particularly about Cremona, a variety of flax called *Lino monochino*, obtained originally, I believe, from Bavaria. It is esteemed much superior to any other, and possesses

possesses a fine silken fibre. It rates in the Italian market much higher than the flax commonly cultivated.—Can you inform me, sir, whether we enjoy this variety?

I have often regretted the want of an *experimental field* for agricultural researches, such for instance as that at Padua, &c. In horticulture we begin to possess this valuable appendix.

I have the honour to be, sir,

Your most humble and very obedient servant,

J. MURRAY.

XXVII. *On the Galvanic Deflagrator of Professor* ROBERT HARE, *M.D. of the University of Pennsylvania, in a Letter to that Gentleman from the Editor of the American Journal of Science.*

Yale College, Oct. 23, 1821.

MY DEAR SIR,—I WAS much impressed by your account of the Galvanic Deflagrator, and of the fine experiments which you performed with it, as described in the third volume (p. 105) of the American Journal of Science*. By means of your kindness in sending me your original apparatus (the only one which, as far as I am informed, has hitherto been constructed) I had it in my power, early in the month of June, to repeat your experiments in my public course of lectures. Large numbers of intelligent persons attended, in addition to the classes, and the results gave great pleasure and satisfaction. My health being at that time very feeble, it was not in my power to pursue the subject to the extent which I had intended; and expecting to resume it, I had postponed the writing of a notice of your instrument, hoping that by and by I could do it more to my own satisfaction. But as no one else appears to have repeated your experiments, I have concluded, even at this late moment, to throw a hasty notice into the Journal, although it has not been in my power to add any thing to the experiments performed in June.

I can say with truth that I consider your Deflagrator† as the finest present made to this department of knowledge, since the discovery of the Pile by Volta, and of the Trough by Cruickshank. The vessels being filled with the fluid, beforehand, prevents any haste or confusion, and the advantage which your arrangement gives the operator, of immersing, at one quick movement, the whole of an extensive series, is very great. Being perfectly ready, and with the poles in his hand, the teacher only giving a signal to his assistant to immerse the coils, instantly directs the whole power to the desired point, and produces results, which, both in

* See Phil. Mag. vol. lvii.

† Your Calorimotor I have never possessed.

brilliancy and energy, totally surpass any thing before effected by the same surface of metal, arranged in the same number of combinations. This will appear the more remarkable, when it is remembered, that your apparatus produces these effects without insulation. Although through your civility I have just received the glass jars by which you insulate your coils, I have not yet been able to use them, and can therefore speak only of the results obtained without them.

With your eighty coils of fourteen inches by six, for the copper, and of nine by six for the zinc, I obtained effects which, as to every thing that related to intense heat and light, and brilliant combustion, far surpassed the powers of a battery of the common form of six hundred and twenty pairs of plates—one hundred and fifty pairs of which, of six inches square, are insulated by glass partitions—one hundred pairs of the same size, and three hundred of four inches square, are insulated by resin, and the rest either by Wedgwood's ware or by resin, making in the whole a battery with a surface of thirty-six thousand eight hundred and eighty square inches. Yours has a surface of only twenty-two thousand and eighty square inches, *but even without insulation* it is incomparably more powerful than the other with that advantage. This is the most singular circumstance connected with your new apparatus, and which goes far to shake our previous theoretical opinions, if not to support your own.

I repeated every important experiment stated in your memoir, and with results so similar, that it is scarcely necessary to relate them. The combustion of the metals was brilliant beyond every thing which I had witnessed before, and the ignition of the charcoal points was so intense, as to equal the brilliancy of the sun; the light was perfectly intolerable to eyes of only common strength. If I were to name any metallic substance which burned with more than common energy, it would be a common brass pin, which, when held in the forceps of one pole, and touched to the charcoal point on the other, was consumed with such energy, that it might be said literally to vanish in flame.

The light produced between the charcoal points when immersed beneath acids, oils, alcohol, ether, water, &c. was very intense, and platina melted in air as readily as wax in the blaze of a candle. It is a very great advantage of your Deflagrator, that we can suspend the operation at any moment, with the same facility with which it was commenced. A look, directed to the assistant, is sufficient to raise the coils out of the fluid. All action instantly ceases—neither the metal nor the fluid is wasting any further, and the lecturer is therefore at ease while he illustrates and reasons; and when he is ready, and not before, he proceeds to his next experiment. In the mean time, the instrument,

ment, during a certain period, rather gains than loses strength, by the raising of the coils. It seems as if the imponderable fluids, partially exhausted from it by its continued action, had time again to flow in from surrounding objects, and thus to impart new energy. I found the power of the instrument to last for several days, although declining, and the same charcoal points, when well prepared *, would also continue to operate for several days. When the coils, after immersion, had been suspended, for some hours, in the air, a coating of green oxide or carbonate of copper always formed on one part of the outside of the copper coils, and on the same part in all, but no where else. If I do not misremember, it collected next to the negative pole, but was, of course, always removed by the next immersion, though it was formed again at the next suspension.

One circumstance occurred during these experiments, which demands further attention.

In the hope of uniting the power of your Deflagrator with that of the common galvanic battery, I connected your instrument with the powerful one mentioned above. Both instruments, *when separately used*, acted *at the time*, with great energy, producing both their appropriate and common effects, in a very decided manner; but, on connecting by the proper poles, the battery of six hundred and twenty pairs, with the Deflagrator of eighty coils, I was greatly surprised and disappointed, at finding the power of both instruments so completely paralysed, that, at the points where a moment before, and when separate, a stream of light and heat, hardly to be endured by the eye, was poured forth—now, when connected, both instruments could scarcely produce the minutest spark. On separating the instruments, they both resumed their activity; on again connecting them, it was again destroyed, and so on, as often as the experiment was made. While they were in connexion, provided the coils were lifted out of the acid, so as to hang in the air merely, then the power of the common galvanic battery would pass through the Deflagrator, which appeared to act simply as a conductor; and, as might have been expected when so extensive a conductor was used, the power of the common battery was, in this case, considerably diminished, while that of the Deflagrator did not act at all.

If, while things were in this situation, the coils of the Deflagrator, without being plunged, were lowered so far as merely to dip their inferior extremities say only one-fourth of an inch in the acid, the communication was immediately arrested, and all effect destroyed almost as completely as when the coils were wholly immersed. Thus it appears that the inability to act, in

* By igniting pieces of mahogany beneath sand in a crucible.

connexion with the common galvanic battery, depends upon the relation of the fluid and metal, and not upon that of the metals merely. These experiments should be repeated, with the aid of the insulating glasses, placed so as to receive the coils of your machine. I should be very curious to know whether the effects would be the same; and as I now have the glasses, I shall, as soon as possible, try this experiment. We must look to you, sir, for the explanation of this singular incompatibility between the two instruments. At present, I confess myself unable to explain it. It may, very possibly, lead to important results, and may have a bearing, such as I have not now time to discuss, on your own peculiar theory.

I would state that the mode of connecting the two batteries was varied in every form which occurred, not only to myself, but to several able scientific gentlemen who were present at these experiments, and who were equally with myself surprised and confounded by their results.

I congratulate you upon the brilliant additions which you have made to our experimental means, in this department of knowledge. Along with your invention of the compound Blowpipe, they fairly entitle you to the gratitude of the scientific world, notwithstanding the uncandid attempts which, in relation to the Blowpipe, I am sorry to see, are still persevered in, to deprive you of the credit which you so richly deserve.

I remain, as ever, your friend and servant,

Prof. Robert Hare, M.D. B. Silliman.

XXVIII. *On Addition and Subtraction of Algebra.* By Mr. Paul Newton.

To Dr. Tilloch.

Old Assembly House, Newark, Feb. 11, 1822.

Sir,—The confused notions which have hitherto prevailed concerning Addition and Subtraction of Algebra, and the consequent inconsistency with which our best authors have treated these rules, incline me to indulge the hope that you will admit, on this subject, a few observations as a supplement to what you kindly inserted in No. 285 of your distinguished and invaluable Magazine.

"*Time*, which overthrows the illusions of opinion," must establish, in its progress, just regulations of *quantity*. I shall again refer to Mr. Bonnycastle's treatise, for instances of injudicious arrangement, not from any invidious motive of detracting from his merit, but because his treatise is, I believe, the last great work on the subject, and because his errors are calculated to mislead,

lead, precisely in proportion to that high degree of estimation in which his writings are held. All Mr. Bonnycastle's three cases, in Addition, exhibit a mixture of positive with negative quantities. Now, this mixture is contrary to the nature of Addition, for its operations should be limited to quantities (whether like or unlike) which are either all positive, or all negative. By avoiding this mixture, Addition will be greatly simplified, and rendered consistent. When positive and negative quantities are opposed to each other, Subtraction must inevitably constitute a part of the operation. Nothing can be more certain than that the "incongruous mixture," in question, should be transferred to the rule for subtracting *simple quantities*, in which the operations will require no change of signs, because only those quantities require to be subtracted, to which the negative sign is prefixed.

A change of signs is applicable to the Subtraction of *compound quantities* only, and to *such only* as contain a mixture of positive with negative terms in the subtrahend; for, when the subtrahend consists entirely of negative terms, Subtraction may then be performed by the rule for *simple quantities*, since the negative terms in the minuend (if any there are) will continue to be negative terms, when transferred to the subtrahend.

To conform to the old rules, out of mere politeness, is to violate reason, in an instance, in which, to exercise reason is our professed purpose. By absurdly admitting a part of Subtraction in Addition, and by confining the nominal rule of Subtraction to a mere change of signs in the subtrahend, our indulgent authors apparently justified each other in the impropriety of prefixing the affirmative sign $(+)$ to a compound quantity, as $13-\sqrt{b}+(5-4-\sqrt[3]{x})$.

But, should future authors perceive the propriety of making those now arrangements which I have suggested; then, either some new sign must be substituted for $(+)$, when it is used as a prefix to certain compound quantities; or, a mose extensive definition must be given to this affirmative sign, than that which is appropriated to it when applied to simple quantities.

It is often fruitless to search for the origin of vulgar errors; but we may with probability suppose that our authors derived their erroneous ideas of Addition, from the operations necessary to be performed in finding the final product of some factors in Multiplication. Thus, suppose it were required to multiply x^2+xy-y^2, by $x-y$.

Here $(x^2+xy-y^2)\times(x-y)=$

$$(x^2+xy)x-(x^2+xy)y-(x-y)y^2=$$

$$(x^3+x^2y)-(x^2y+xy^2)-(xy^2-y^3)=x^3-2xy^2+y^3,$$

which

which final product, instead of being the result of Multiplication and *Addition*, I entertain no doubt, sir, that all your readers will perceive, is obtained by Multiplication and *Subtraction*.

I have the honour to be, sir,
Your most obedient humble servant,
Paul Newton.

XXIX. *On Flame, &c.* By John Murray, *F.L.S. M.W.S. &c. &c.*

To Dr. Tilloch.

London, Feb. 7, 1822.

Sir, — I have elsewhere combated the opinion of Sir H. Davy, touching the structure of flame. In my chemical Prælections I was under the necessity of examining the very ingenious and novel views promulgated by this justly celebrated chemist, and of recording my dissent from some of these inferences. *Inter alia*, I contended that flame in common circumstances was to be considered a *superficial film*, and in this position my numerous experiments are quite conclusive and bear me out. I take leave to quote a passage from Lord Bacon's *Sylva sylvarum*, interesting, as it shows that the opinion of Sir H. Davy was entertained even by that great master of the Philosophy of Induction:

"Sume ceream et statue in tubulo ferreo aut æreo,—postea impone illum erectum scutellæ, spiritu vini plenæ et calefactæ, deinde cerea et spiritu vini simul igni impositis, flammam ceræ dilatari videbis et quadruplo quintuploque intermiscere, quam ante soleret; apparetque in rotunda non pyramidali figura. *Insuper internam cereæ flammam conspicies* servato colore, neque quicquam cærulei contrahere versus colorem externæ flammæ in spiritu vini, &c."

This experiment Lord Bacon calls "*egregia instantia.*" Now, it is, on the other hand, a *noble example* of what leads to a conclusion the very reverse of that inferred. If the flame of the alcohol envelops that of the taper, the latter is *invariably extinguished.* This fact is best exemplified by using only a limited surface of alcohol, for when a larger quantity is employed the apex of the flame is ragged and uneven, and does not unite in a conical form from the resisting and undulating atmosphere which, therefore, fills up the chasms. The wax of the taper melts down, and affords an additional proportional of carbon to the vapour of the alcohol, thus imparting to the summit of the flame in its transit an increased illuminating power.

It is singular that in a subsequent page, our author under "Experimentum solitarium spectans flammam," &c. describes a phænomenon

a phænomenon which is fatal to the inference he had just before deduced, and conclusive with respect to the opinion I have presumed to maintain.

"Saggittam flammæ impositam tene durante spatio decem pulsuum, ea deinde exempta comperies *partes saggittæ exteriores versus flammam* magis *ustulatas et nigricantes* maximaque ex parte in carbonem versas; *cum in medio* quod fuit, videatur *duntaxat libatum igne*," &c.

This last is a fine experiment; and if properly managed, the splinter of wood or other material may be withdrawn from the flame, the *central part untouched*.

In reference to *aphlogistic* phænomena in the article "COMBUSTION," in Dr. Ure's Nicholson's Dictionary of Chemistry, the following occurs: "Platinum and palladium, metals of low conducting powers and small capacities for heat, *alone* succeed in producing these phænomena." Now I am confident Sir H. Davy would not wish a compliment paid to him at the expense of *truth*, and on these terms I am also certain Dr. Ure would not deire to bestow it. Sir H. Davy not finding other metals "succeed in producing these phænomena," was perfectly justified in maintaining this opinion in his beautiful Researches on Flame. But in 1819, I mentioned (see Annals of Philosophy) that Sig. Sementini at Naples had found that *silver and copper* (metals of *high* conducting powers in relation to heat) exhibited aphliogistic phænomena as well as platinum, &c. I would therefore simply put the question, "Did not Professor Sementini show Sir H. Davy these experiments during his sojourn at Naples, as well as to myself?"—I presume this to satisfy Dr. Ure.

Sig. Sementini was so good as to favour me with a portion of *silver wire* the size he had found successful; and on my return to Paris, I showed the experiments with the *silver rings* reposing on *camphor* to Mons. Robiguet and other Sçavans, and in this country also to several of the Professors of the University of Aberdeen. I have the honour to be, sir,

Your most humble and obedient servant,

J. MURRAY.

XXX. *On setting Cutting Instruments.*

THE thanks of the Society for the Encouragement of Arts, Manufactures, and Commerce, were last Session voted to George Reveley, Esq. of Queen-square, for a communication on the use of soap instead of oil in setting cutting instruments on a hone. It sets quicker, gives a good edge, removes notches with great facility, and is a much more cleanly material than oil. The operation is performed as follows:

Having

Having first cleaned your hone with a sponge, soap and water, wipe it dry; then dip the soap in clean soft water, and wetting also the hone, rub the soap lightly over it, until the surface is thinly covered all over; then proceed to set in the usual way, keeping the soap sufficiently moist, and adding from time to time a little more soap and water, if it should be necessary. Observe that the soap is clean and free from dust before you rub it on the hone; if it should not be so, it is easily washed clean. Strop the razor after setting, and also again when you put it by, and sponge the hone when you have done with it.

The preference due to Mr. Reveley's method over the use of oil is certified by practical gentlemen; viz. Messrs. Wm. West, W. H. Pepys, Richard Long, and Isaac Fremor.

A paste or powder for razor strops, very superior to emery, plumbago, and other things commonly used, has been discovered in Paris by M. Merimée. It is the crystallized tritoxide of iron, called by mineralogists *Specular Oligiste Iron.* It is a mineral substance, but an artificial oxide of equal fitness for the purpose may be made thus: Take equal parts of sulphate of iron (green copperas) and common salt. Rub them well together, and heat the mixture to redness in a crucible. When the vapours have ceased to rise, let the mass cool, and wash it to remove the salt, and when diffused in water, collect the brilliant micaceous scales which first subside. These, when spread upon leather, soften the edge of a razor, and cause it to cut perfectly.

XXXI. *Answer to the Question addressed to the Reverend* J. Grooby *in our last Number.* [See p. 50.]

To Dr. Tilloch.

Cirencester, Feb. 12, 1822.

Sir, — I beg to inform the gentleman who asks, From what tables of M. Bessel I took the corrections for Dr. Maskelyne's stars? that it was from the same he alludes to; namely, from those annexed to the first part of the Konigsberg observations. From the specimen your correspondent has given of *his* method of ascertaining the corrections, I am not at all surprised at his numbers differing from my own, nor at his hence concluding that I had fallen into some error; but I am rather surprised that his self-confidence should have led him to make the strange and unwarrantable assertion, that the Professor has not made use of his own tables in reducing his observations.

Your correspondent tells us that, in every instance in which he has used the tables, he has found his results to differ, not only from those of another individual, but also from those of the author himself; and he hence concludes, not very modestly I must say,

say, that M. Bessel does not use, or does not know how to use, his own tables.

To be brief: The error of your correspondent has arisen from his supposing the increase or decrease of the differences to be regular, and calculating accordingly. If instead of taking a *mean proportional part* of the difference, he will find the *true* difference, by interpolation; for Dec. 14. 56 he will get 0,026, which subtracted from +0.273, gives +0.247, the very same M. Bessel has given in his example. If in the same way he will recalculate the "some hundreds of observations," which he tells us he has reduced, I have no doubt it will lead to a conclusion very different from that he has so rashly adopted. I will also hope that, calculating in this way, he will find that the tables *do* give the same corrections I have made use of.

I am, sir, your obedient servant,

JAMES GROOBY.

XXXII. *Observations on the dangerous Rock usually called The Drunken Sailor, lying off the Flag-Staff Point, Colombo, Island of Ceylon.* By *Lieut. Col.* GEORGE WRIGHT*.

THE above rock, usually called by the English The Drunken Sailor, and by the Dutch *De Dronke Matroos*, lies in a direction by compass about *west-south-west* from the Flag-staff of Colombo, and distant from a bold projecting rock usually named the Portuguese Rock, on the sea shore directly in front of the Flag-staff about *three quarters of a mile*. Its situation is in a most dangerous position, being exactly in the track that a ship would make in trying to reach the anchorage in the roads of Colombo during the north-east monsoon, and at which time it may be considered as most dangerous, from the circumstance of the sea not making any break upon it, which is the case during the south-west monsoon, when breakers are distinctly seen at intervals, and which in general sufficiently mark its position: but even then it is not always visible, as at times only a small white surge scarcely discernible can be perceived to rise over it once in seven or eight minutes.

Upon the summit of the rock the greatest depth of water which has as yet been ascertained, is about *six* feet; and the smallest about *three feet and a half*, that being the usual difference of the tides on this coast, or rather the difference of level in the sea caused more by strong southerly winds than by the tides, which at Colombo do not reach two feet. The summit of the rock is very small, and appears to be of an oval shape, of about *twenty* or *thirty* feet in circumference, and the sides

* From the Transactions of the Ceylon Literary Society.

of the rock exceedingly steep and abrupt; the depth of water at a few yards distance, from *nine* feet to twenty-five; and a little further off to about nine fathom, which is the greatest depth of water between the rock and the shore; the rock itself appears to be of a sharp and hard-kind, much indented, and full of crevices, as small anchors or grapplings which have been made use of by boats to anchor on it, as well as the leads used in sounding the depth, have in general been extricated therefrom with much difficulty, and from the circumstance of the rock not appearing to increase in magnitude, it is most probably not of the description of coral rock so frequent in the Indian sea.

Although alluded to and taken notice of in some old Dutch manuscript charts and surveys, this rock appears to be but very little known in general, and few, if any, of the English charts take notice of it at all. One of the latest editions of that valuable work of Captain Horsburg, Hydrographer to the Honourable East India Company, mentions it; but as the same is contained in an appendix to the second volume of the work, the circumstance there is no doubt often escapes observations. A transport with troops making the roads of Colombo in the year 1819, passed within a short distance of it, not aware of the danger; and some years since a large and valuable East Indiaman stood close in shore and tacked several times close to it, and passed between it and the shore without being aware that such a rock existed.

Colombo, Aug. 8, 1821.

XXXIII. *Account of an improved Method of planting Vines for Forcing.* By Mr. DANIEL JUDD, *F.H.S.**

HEREWITH I send an account of my management of the vines in the garden of Charles Campbell, Esq. of Edmonton, of which I have the charge.

My compost was formed as follows: In the winter of 1817, I procured a quantity of the top-spit of soil from a common in the neighbourhood, which consisted of a rich loam, rather inclining to be gritty, which property I prefer, because it gives a porousness to the compost, thereby allowing the water to pass freely through it. At the same time I collected some lime rubbish, well broken to pieces and sifted, some old tan, some leaf mould, and a quantity of the richest old dung I could select from the forcing-beds and elsewhere.

These materials having been kept separate, and frequently turned over in the summer, were mixed together in the autumn

* From the Transactions of the London Horticultural Society.

of

of 1818, in the following proportions: one-half of loam, one-fourth of dung, and one-fourth of lime rubbish, united with the tan and leaf-mould. They were well mixed, by frequent turnings (but were not sifted) during the winter, when the weather was frosty or dry, for this operation should never be performed in wet weather.

It may be noticed, that I did not use so much dung in my compost as is sometimes done; for I have observed that an excess of it retards the growth of the vine, notwithstanding it is considered to be a plant which will bear an extraordinary quantity of manure. The addition of old tan to the compost, which is not usual, I recommend, because I know, from experience, that the vines will root in that more freely than in any other substance.

In March last, the border, in front of the vinery, was cleared to the depth of upwards of three feet, below which it was drained, and then filled up with the new compost to the level of the bottom plate of the house; this was done in fine weather, and the new mould had full two months time to settle well before the young vines were planted in it.

My vine plants were raised from single eyes in March 1818; they were treated in the usual way through the summer, and kept from the frost during winter, until March last, when they were cut down to one eye, and placed in the pine-pit in order to produce young shoots of sufficient length to draw into the house at the time of planting. After they had made shoots about two feet long, they were removed to the green-house (which was at that time kept at a temperature of about 60°, for some other purposes); here they continued growing, till they had attained to the length of three or four feet; by this treatment the whole plant was rendered more hardy, and consequently more fit for its final removal into the open border.

Early in May, having made good the height of the border quite to the level of the holes where the plants were to be carried into the house, so that no part of their stem should be exposed to the external air, I opened the holes, for the reception of the plants, leaving them open upwards of a week, to remove any noxious quality in that part of the compost which would first receive the roots.

My planting was executed on the 13th of May; but I consider that any period between the 10th of May and 10th of June will be equally successful, provided the work be done in seasonable weather, that is, when it is neither wet nor cold.

At the time of planting, I turned into each hole, a common wheel-barrow full of very old tan from the pine-house, in the middle of which tan the roots of my vine plants remained after

the plants had been treated as I shall now describe. I first cut off the leaves from the lower part of the plant, about two feet and a half of its length, leaving about an inch of the footstalk of each on the plant, the end of which was then drawn very carefully through the hole, under the plate, without injuring the tender part of the shoot; the pot being removed, the ball or root of the plant was placed two feet distant from the front of the house, upon its side, so that the stem lay in a horizontal position, about six inches below the level of the surface of the border. When thus placed, the whole of the stem which was to be covered was slit or tongued, at each eye, like a carnation layer, by passing a sharp knife at three-quarters of an inch below each eye, and on the side of the eye, about one-third of the thickness into the wood, and then upwards to the centre of the joint. This being done, the stem was covered with about four inches of old tan, and the other two inches were filled up with the mould of the border. It is essential to the safety of the plant that the slitting be done the last thing, and whilst it is laid in its position, lest the stem should be broken.

The effect of the operation of slitting the stem is the production of abundance of roots from every eye; the progress is not very great until the roots begin to push out: after these shoot, it is surprising how fast the vines grow.

I gave a little fire in the house for the first month after planting, though sparingly, and air was admitted into it continually, until the plants had got sufficient hold of the border; air was then admitted in the day, but the house was shut up at night. Under this treatment, the shoots of the present season of these young plants are from twenty-five to thirty feet long, and their strength is fully proportionate to their length.

It is not my intention to grow any thing on the border, which will exhaust it, or deprive the vines of their full nourishment. To protect their roots in the winter, I shall use a covering of old tan, about six inches thick, which I prefer to dung or mulch of any description.

I have this season planted vines in the same way, in other houses, besides the one I have now mentioned, and with equal success.

XXXIV. *Report from the National Vaccine Establishment. To the Right Honourable* ROBERT PEEL, *Principal Secretary of State for the Home Department.*

National Vaccine Establishment, Percy Street, Jan. 31.

SIR,—VACCINATION has now been submitted to the test of another year's experience, and the result is an increase of our confidence

fidence in the benefits of it. We are happy to say that it appears to have been practised more extensively than it was, notwithstanding the influence of exaggerated rumours of the frequent occurrence of small pox subsequently, on the minds of some persons, and the obstinate prejudices of others, who still continue to adopt inoculation for that disease. The unavoidable consequence of the latter practice is to supply a constant source of infection, and to put the merits of vaccination perpetually to the severest trial.

Of small pox, in the modified and peculiar form which it assumes when it attacks a patient who has been previously vaccinated, many cases indeed have been reported to us in the course of last year, and some have fallen within the sphere of our own observation; but the disorder has always run a safe course, being uniformly exempt from the secondary fever, in which the patient dies most commonly when he dies of small pox.

For the truth of this assertion, we appeal to the testimony of the whole medical world; and for a proof that the number of such cases bears no proportion to the thousands who have profited to the fullest extent of security, by its protecting influence, we appeal confidently to all who frequent the theatres and crowded assemblies, to admit that they do not discover in the rising generation any longer that disfigurement of the human face which was obvious every where some years since.

To account for occasional failures, of which we readily admit the existence, something is to be attributed to those anomalies which prevail throughout nature, and which the physician observes, not in some peculiar constitutions only, but in the same constitution at different periods of life, rendering the human frame at one time susceptible of disorder from a mere change of the wind, and capable at another, of resisting the most malignant and subtile contagion. But amongst the most frequent sources of failure which have occurred, and will for a time continue to occur, is to be numbered that careless facility with which unskilful benevolence undertook to perform vaccination in the early years of the discovery; for experience has taught us, that a strict inquiry into the condition of the patient to be vaccinated, great attention to the state of the matter to be inserted, and a vigilant observation of the progress of the vesicles on the part of the operator, are all essentially necessary to its complete success.

That less enlightened parents should hesitate to accept a substitute for inoculation, which is not perfect in all its pretensions, and absolutely and altogether effectual to exempt the objects of their solicitude from every future possible inconvenience, does not surprise us: but we cannot forbear to express our unqualified reprobation of the conduct of those medical practitioners, who, knowing

knowing well that vaccination scarcely occasions the slightest indisposition, that it spreads no contagion, that in a very large proportion of cases it affords an entire security against small pox, and in almost every instance is a protection against danger from that disease, are yet hardy enough to persevere in recommending the insertion of a poison, of which they cannot pretend to anticipate either the measure or the issue, (for no discernment is able to distinguish those constitutions which will admit inoculated small pox with safety), and there are some families so dangerously affected by all the eruptive diseases, that they fall into imminent hazard in taking any of them. This remark has a particular application to small pox. A family lost its two first-born children of the small pox, inoculated by two of the most skilful surgeons of the time: nor is it improbable that the parents might have had to lament the loss of more children under the same formidable disease, if the promulgation of the protecting influence of vaccination had not happily interposed to rescue them from the consequences of a repetition of the fatal experiment. Of their remaining children, one took the small pox after vaccination, and went through it in that mild and mitigated form which stamps a value upon this resource, as real in the eye of reason and sound philosophy, as when it prevents the malady altogether.

We have contended, Sir, for this its merits, with all the powers of our understanding, and with all that just and fair pretension to convince others, to which we are entitled by being firmly and sincerely convinced ourselves. Nor shall we relax in our efforts to promote its adoption, but continue to exert the influence which the benevolent designs of Parliament, in establishing this Board, have given us for extending the benefits of this salutary practice.

That the blessing is not yet absolutely perfect, we are ready to admit; but when we compare it with inoculation for the small pox, the only alternative, we have no hesitation in stating, that the comparison affords an irresistible proof of its superior claims to regard; for we learn from ample experience, that the number of cases of small pox, in the safe form which it is found to assume after vaccination, is by no means equal to the number of deaths by inoculation; an evidence quite irrefragable, and, as it appears to us, decisive as to the incalculable advantages of the practice of the first over that of the latter method.

The number of persons who have died of small pox this year within the bills of mortality is only 508; not more than two thirds of the number who fell a sacrifice to that disease the year before: and as in our last report we had the satisfaction of stating that more persons had been vaccinated during the preceding than in any former twelve months, we flatter ourselves that this diminution

nution of the number of deaths from small pox may fairly be attributed to the wider diffusion of vaccination.

(Signed) HENRY HALFORD, President.

ALGN. FRAMPTON, THO. HUME, CHARLES BADHAM, ROBERT LLOYD, } Censors of the Royal College of Physicians.

EVERARD HOME, Master of the Royal College of Surgeons.

WILLIAM BLIZARD, HENRY CLINE, } Governors of the Royal College of Surgeons.

By order of the Board,

JAMES HERVEY, M.D., Registrar.

XXXV. *On some Compounds of Chrome.* By M. GROUVELLE*.

Acid Chromate of Potash.

ACID chromate of potash is anhydrous: I obtained it by digesting the neutral chromate of potash with nitric acid, separating from the first crop of crystals all those of nitre, and then redissolving and again crystallizing the chromate. When this salt is strongly calcined it melts, and passes to the state of neutral chromate, giving up half its acid, which is decomposed, and leaving an oxide of chrome crystallized in brilliant green scales. The neutral chromate thus obtained was analysed by a solution of sulphurous acid, which changed it instantaneously into sulphate of potash, sulphate and sulphite of chrome. The metallic oxide was precipitated by ammonia, and the sulphate of potash evaporated. The super-chromate, therefore, contains twice as much acid as the neutral chromate, and is composed of

Chromic acid (two atoms)	68·846
Potash (one atom)	31·154
	100·000

Carbonate of Chrome.

On pouring sulphurous acid and potash into liquid chromic acid, M. Vauquelin obtained a brown precipitate, which he thinks is an oxide more oxygenated than the green oxide of chrome. This, however, is not an oxide, but a carbonate of chrome. It dissolves without effervescence in diluted acids. When boiled in distilled water it is decomposed, and the green oxide and carbonic acid gas are obtained, on which account care should be taken not to wash it with hot water. This salt may also be procured in another way, that is, by passing a current of nitrous

* From the *Annales de Chimie et de Physique.*

gas

gas and air through chromate of potash mixed with an alkaline carbonate. Carbonate of chrome then falls down on boiling the mixture; but if it contains too much nitrous acid, the whole will pass to the state of nitrate of chrome. This method, however, often fails: it is evidently the nitrous acid which reduces the chromic, and the oxide thus produced attracts to itself the carbonic acid driven from the carbonate by an excess of acid. A better method of obtaining this carbonate is, to evaporate to dryness, a mixture of nitrate of ammonia, chromate, and carbonate of potash; or of muriate of ammonia, with a nitrate, carbonate, and chromate of alkali. This mixture, when gently dried, blackens; it is then to be re-dissolved in water, and a drop or two of ammonia, which has the effect, I believe, of separating a small quantity of carbonate of chrome which the nitrate of ammonia had retained in solution.

If too high a degree of heat be applied, the excess of nitrate will re-produce the chromate. Here it is the protoxide of azote (nitrous oxide) in its nascent state which decomposes the chromic acid; for, when once become gaseous, it has no longer this property. If, on the other hand, the chromate of potash and the nitrate of ammonia are acidified with nitric acid, and dried and heated in a tube protected from the contact of air, no carbonate of chrome whatever is obtained.

A mixture of nitre and muriate of ammonia acts in the same manner as nitrate of ammonia, because a double decomposition takes place, on account of the facility with which the nitrate of ammonia assumes the gaseous form. This double decomposition always occurs when these salts are heated with the nitrate of any metal capable of forming a fixed chloruret with muriate of ammonia.

Therefore, to obtain nitrous oxide, instead of employing caustic nitrate of ammonia, we may use nitrate of potash and muriate of ammonia, in the proportions suited to complete decomposition, leaving, however, an excess of nitrate, to avoid any sublimation of the sal-ammoniac. The proportions may be about three parts of nitre to one of sal-ammoniac.

Chromites.

The existence of these salts is still doubtful, and Berzelius has not yet ventured to admit them positively in his *System of Mineralogy*. However, Vauquelin obtained a precipitate by pouring chromate of potash into proto-sulphate of iron, which he has found to be composed of oxide of iron and oxide of chrome, and is analogous to the chromic ore of the Var, particularly when the latter is calcined. Other chromites may be obtained with the muriates of manganese and of tin with oxide of chrome.

That

That with tin is green; with manganese chesnut-brown. They have all very similar properties: they dissolve in acids, and are precipitable from them without decomposition; the chlorate and nitrate of potash change them into alkaline chromates, and metallic oxides. I have tried, but without success, several methods of separating them by analysis. With chlorate of potash they undergo a combustion similar to that of nitre and cream of tartar. A soluble chromate is indeed obtained, but the oxides of iron, of manganese, or of tin, retain much of the chromic oxide. Muriate of chrome renders muriate of manganese very soluble in alcohol; caustic alkalies cannot separate the whole of the oxide of tin from the oxide of chrome. These compounds deserve a fuller examination.

Chromate of Lead.

It is well known that a reddish chromate of lead is obtained by precipitating acetite of lead with an alkaline chromate of potash; but if the sub-acetite of lead and neutral chromate are used, both boiling hot, a yellow precipitate falls down, which in a few moments passes into a most brilliant orange-red. This tint may be heightened by boiling a little alkali with the red, or even with the yellow chromate of lead. I have made a comparative analysis of the yellow and the red artificial chromate, and the native red lead of Siberia. All of them give exactly the same proportion between the acid and the oxide. They are neutral chromates, only the red chromate contains a small quantity of alkali, apparently from 1 to 1½ per cent. The method which I used in these analyses was to dissolve the chromate in muriatic acid, which in a boiling heat became muriate of chrome; then to precipitate the lead by sulphuretted hydrogen, the oxide of chrome by ammonia; and lastly to evaporate to obtain the muriate of potash. All the alkalies will change the fine yellow of the chromate of lead, and also of bismuth, into red.

It remains to inquire whether the alkali is combined with the chromic acid, the oxide of lead, or the chromate of lead. For this purpose, I treated a very pure chromate of lead and bismuth in excess with a small quantity of alkali, assisting the action by heat. After some instants the liquid had ceased to redden turmeric, and had assumed a yellow tint. Sometimes the turmeric test showed the absence of free alkali before the liquid changed colour, at which time the chromate contained free oxide of lead. Indeed, if a little litharge is added to the chromate along with the alkali, it will become red without losing chromic acid. One may even obtain red chromate by boiling together chromate of potash and litharge.

It follows from these facts that the alkali appears to be com-

 bined

bined with the oxide of lead; and that this compound, united to chromate of lead, gives rise to the red chromate, which thus contains a little more oxide of lead than the neutral chromate. A few drops of dilute nitric acid take away from it immediately its red colour, by dissolving the alkali with a little of the oxide of lead. I examined whether the red lead of Siberia, which is also yellow when reduced to powder, might contain a portion of alkali; I found in it, after taking every precaution, a little lime, but I am ignorant whether or not it is accidental.

XXXVI. *Description of the Methods employed in determining the Altitudes of several of the principal Mountains and other remarkable Objects visible from the Trigonometrical Station on Rumbles Moor, Yorkshire.* *By* A CORRESPONDENT.

To Dr. Tilloch.

PREPARATORY to taking the field in the spring of last year, to collect the requisite data for determining the abovementioned altitudes, I deemed it advisable, so slender was my stock of information on the subject of terrestrial refraction, to make, during the winter, daily observations of the apparent altitude of a mountain, of which the elevation as well as that of the place of observation could be readily determined by levelling. Rumbles Moor and an observatory 67,082 feet distant, both situated within three miles of a canal communicating with the Irish sea, were ultimately made choice of.

The instruments made use of in measuring the angles, were two horizon sectors, of which the following is a brief description:

The one first used consists of a 30-inch achromatic telescope *a* (see Plate II.) fixed in the hollow square frame of mahogany, *b*; to one of its vertical sides is attached a plate of brass, *c*, containing 10 degrees of elevation and an equal quantity of depression. The radius of the arc is nearly 18 inches, admitting the divisions to be read off to 5″ by the moveable index *d*, which carries with it the adjustable spirit-level *e*. When the line of vision is known to be parallel to the plane of the horizon (the index being at zero) the bubble of the level is adjusted to remain in the middle. The small cross level *f* determines the vertical position of the divided plate, and the line of collimation is rendered parallel to it by means of a proof telescope. When the object is elevated or depressed, the corresponding angle is measured by the index *d*, properly *levelled*; that is, moved by the pinion (*h*) until the displaced bubble is again in the middle. Granting the interior sides of the glass level parallel to each other,

other, it follows that the mean of two observations carefully made with the instrument *erect* and *inverted* would give the correct angle, and render the adjustment of the level superfluous. To ascertain and determine the value of the inclination of the sides of the glass tube, two further observations with the spirit-level taken off and *reversed*, immediately succeeded, and the mean of the four readings considered as the true angle. The level being curved proved so very unmanageable with its concave side uppermost, that the instrument was shortly laid aside for the horizon sector, No. 2, which will be best understood by describing the method of verifying its adjustments.

Having placed the instrument upon an immoveable stand, fix the point of intersection of the cross wires of the 20-inch brass telescope (*c*), Plate II. upon some distant well-defined object by means of the rack-work (*d*), and the clamp (*e*) of the brass stand (FG), and the intersection should remain perfect during an entire revolution of the telescope in its Ys (*hh*) (17 inches asunder). Then throw open the semicircular rings (*a b*), and having placed the left hand index (*i*) at zero on the limb (*j*) by the rack-work (*k*) and the pinion (*l*), cause the bubble of the level (*m*) to appear at its mark by altering the inclination of the telescope as before. When properly adjusted, the telescope will bear being reversed in its Ys without displacing the bubble. Lastly, *invert* the telescope and repeat the verifications for the *right hand* limb, index and level (*n*). During the operation the limbs should be rendered vertical by their respective cross levels (at *n* and *o*). When the adjustments are not perfect they are rendered so, in the usual manner, by the adjusting screws of the cross wires and of the levels.

The divisions (on silver) have a radius of 15 inches and can be read off to five seconds.

Both the instruments were made and divided by the late Mr. James Allan (Fetter-lane) from models sent to him.

In making observations the instrument is placed upon a tripod stand, or, what is preferable, upon a rock or pile of stones, and the telescope accurately pointed and clamped upon the object. The left hand limb is *afterwards* (which is a great advantage) rendered vertical; the index levelled, and the angle read off by the elevation as well as by the depression side of the zero of the vernier (of the index), and a *proper* mean registered. To verify the adjustment of the telescope it is now *inverted*, again clamped upon the object, the *right-hand* index levelled, and double readings repeated. The mean of the four angles is considered as the correct one.

At the *observatory* a greater degree of accuracy was obtained by *reversing* the telescope at each observation, and reading off

the angle on the other side of zero, the indices being first properly levelled. Eight readings were thus obtained, and the errors of collimation, dividing, &c. reduced to a mere trifle. By this method it was also discovered that the instrument being adjusted at 52°, the zenith distances would be 10″ in defect so soon as the thermometer had fallen to 32°. Hence the necessity of noting the temperature of the sector at the time of adjusting it, and also after every pair of observations. The adjustments, which did not require altering more than three or four times in the course of the year, are best made at 40° in winter and at 60° in summer.

When placed upon a distant object, the cross wires seemed absolutely to efface it, and to render great accuracy unattainable. A filament scarcely visible even in its magnified state, and accidentally found adhering to the cross wires in the manner exhibited in Plate II. was successfully substituted, and the point (*a*) considered as the line of vision.

Before any remarks are made as to the result of the experiments on terrestrial refraction, it will be proper to state that twelve observations were made at the observatory in January; thirty-two in February; sixty-one in March; and eighty-six from the 1st to the 18th of April; and that the instrument was afterwards taken to the following stations:

Rumbles Moor.

1821.			h. m.		h. m.	Ther. in shade.	Wind.	Bar.
April 23,	7	obs. from	10 20	to	11 30	48 to 50	N.E.	
24,	6	.. ..	17 25	..	18 10	48 .. 52	S.S.W.	
25,	24	.. ..	10 45	..	17 45	60 .. 65	E.	
28,	6	.. ..	11 10	..	12 35	51 .. 56	W.	
30,	38	.. ..	9 30	..	18 45	42 .. 54	N.N.E. and N.N.W.	
June 2,	42	.. ..	11 5	..	19 30	48 to 63	E.N.E.	
19,	23	.. ..	11 5	..	19 20	47 .. 50	N.N.E	28·855
Aug. 7,	10	.. ..	11 20	..	17 0	55 .. 59	S.W.	
Oct. 15,	11	.. ..	13 15	..	14 20	48 .. 50	W.	

Beamsley Rock.

May 2,	33	.. ..	10 15	..	18 0	55 to 67	S.S.E.	
June 14,	30	.. ..	11 30	..	18 30	51 .. 64	E.S.E.	28 925
27,	12	.. ..	16 30	..	19 50	46 .. 56	N.N.E.	
Oct. 30,	9	.. ..	13 5	..	16 25	42 .. 44	W.S.W.	

Chevin.

Aug. 13,	37	.. ..	11 30	to	18 40	57 .. 65	S.S.W.

Great

Great Almias Cliff.

1821.		h. m.	h. m.	Therm.	Wind.	Bar.
Aug. 15,	15 obs. from	12 30	to 16 30	—62 to 64	W.	

Jack Hill.

Aug. 17,	8	16 0	to 17 0	—57	S.W.	

Symon Seat.

Aug. 29,	8	15 20	.. 16 40	—52 .. 53	S.E.	28·417
Sept. 8,	9	14 0	.. 15 50	—56 .. 57	S.W.	

Great Whernside.

Sept. 1,	9	12 10	.. 13 45	—52 .. 56	N.W.	

Pendle Hill.

Sept. 24,	12	10 50	.. 14 20	—48 .. 51	W.N.W.	28·050

1822. *Alfred Castle.*

Jan. 26,	17	11 0	.. 13 15	—40 .. 41	W.N.W.	

The first fact elicited by these numerous observations was the existence of a species of diurnal variation of refraction not exceeding 60″ to 70″ within the limits of the survey, and dependent on the *locale* of the station, the time of the day, and in some degree (if it may be so termed) on the constitution of the day itself. It was first noticed at the observatory, Jan. 25, 1821, and might be said to be at its maximum in March, at which period it exceeded 60″. At Rumbles Moor it was still more marked up to the 3d of June, when it totally ceased. No observations ulterior to those just mentioned, were made at the observatory in 1821; but such angles as have been taken in January last, give no indications of its return. It is also worthy of note (and a comfortable discovery it is for the surveyor) that the mean of the diurnal extremes differs but very slightly from the *constant* angle; generally speaking, the variation is greatest when the mornings are frosty, and the sun acquires great power during the middle of the day. This led me to suspect, that in spite of the sector being reversed after every observation, the variation might be wholly attributed to the change of temperature as affecting the instrument. Observations made on frosty mornings with the sector at 30°, and afterwards heated to 60°, proved by their near agreement that the cause of the variation could not be looked for there. As a further and irrefutable confirmation, the refraction was constant at Beamsley Rock, May 2, (on which day the thermometer had an extensive range,) although the variation continued to be observable at Rumbles Moor. It is to be

be remarked that the nearer the ray passes to the ground, the greater the variation. In general, the refraction when variable is greatest near sun-rise and sun-set, and least during the heat of the day. Even when the diurnal variation is scarcely perceptible, a very sudden increase of refraction of 10″ or 20″ will be remarked on an evening within a short time of sun-set. I have had no opportunities of determining whether the refraction remains at its maximum or not during the night.

At none of the other stations could any certain proofs of the existence of this diurnal refraction be established; but then it must be recollected, that its effects had already ceased at Rumbles Moor, and most probably at the observatory. It might not have been witnessed at Beamsley Rock on account of the sides of the mountain being excessively steep in almost every direction, and perhaps from the extreme dryness of the surface.

In attempting to account for this peculiar refraction, it was in the first place conjectured that the superior strata of the atmosphere might be heated at sun-rise and sun-set in a greater degree than the inferior ones.

Many observations made with the thermometer at the base and summit of Rumbles Moor, tended rather to refute than to confirm this hypothesis. It could scarcely be occasioned by the morning and evening frosts, or its effects would have been again perceptible in the autumn. There is little doubt, however, that the stratum of air immediately in contact with the surface of the ground is hotter about noon, and undoubtedly colder at morning and evening than the succeeding one; but why this irregularity should be confined to five months in the year is not quite so explicable. It serves however to account for the non-existence of the variation on a steep craggy mountain, such as Beamsley Rock.

Observers have generally remarked, at one time or other, cases of sudden and extraordinary refraction; but the following is the only marked one that has come under my notice:—February 9, 1821, the moor appeared under an angle of elevation of 42′ 30″ at 17^h 15^m; yet in the course of a quarter of an hour it was found increased to 48′ 18″. The thermometer, which was then at 41°, fell very rapidly, and shortly after rose as abruptly. The sun's vertical diameter was unusually contracted, and its contour curiously indented; at 10^h of the same day the thermometer was at 41 on the moor (then invisible) and only at 35 at the observatory.

The following observations will (with one exception) serve to verify the theory of the refraction being affected by an *unusual* difference of temperature at the two stations, and will also render the diurnal variation more intelligible. They point out, moreover,

moreover, the advantage of noting the thermometer at the base as well as at the summit of the mountain where the observations are made, and of rudely determining their difference of altitude by the barometer.

(*Height of Eye,* $4\frac{1}{2}$ *feet.*)

At Rumbles Moor, April 30*th.*

h.	m.					Therm.
14	30	observatory depr. ..		53	37	54
15	20	..	..	53	25	$53\frac{1}{2}$
15	40	..	..	53	10	$53\frac{1}{2}$
16	30	..	..	53	5	51
17	0	..	..	53	8	$50\frac{1}{2}$
17	30	..	..	52	54	49
18	0	..	..	52	35	47
18	30	..	..	52	27	45

At Rumbles Moor, June 19*th.*

13	20	observatory depr.		53	14	$48\frac{1}{2}$
14	30	..	..	53	13	$49\frac{1}{2}$
17	20	..	..	53	13	$49\frac{1}{4}$
18	40	..	..	53	4	$47\frac{1}{2}$

The mean of the observations in 1821 was 53.0.

At the Observatory, March 30, 1821. *Rumbles Moor elevated.*

Hour. h.	m.	Barometer.	Wind.	Angle of Elevation. ′	″	Therm.	Therm. at Moor.
10	0	29·52	S.W.	42	6	44	39
10	30	..	..	42	5	43	41
11	0	..	..	42	4	46	40
11	30	..	..	42	4	47	$41\frac{1}{2}$
12	0	29·33	..	42	3	48	41
12	30	..	..	42	3	47	39
13	0	..	..	42	1	47	43
13	30	..	..	42	2	49	41
14	0	29·32	..	42	2	50	41
15	0	..	..	42	4	50	43
15	30	..	..	42	4	49	$41\frac{1}{2}$
16	0	..	..	42	11	49	41
17	0	..	..	42	9	49	41
17	30	..	..	42	10	48	39
18	0	29·26	S.W.	..		46	36

The mean of the extremes is about 42′ 6″ and the mean difference of the temperature of the stations 7°, that of the Moor being the lowest. The temperature of the vapour by Daniell's hygrometer was 8° *minus* that of the atmosphere.

At

At the Observatory, April 5, 1821. *Rumbles Moor elevated.*

Hour. h.	Barometer.	Wind.	Angle of Elevation. ′ ″	Therm.	Therm. at Moor.
5½	29·19	W.S.W.	42 30	35	
6	..	W.	42 44	35	
6½	..	W.N.W.	42 30	35½	
7	..	do.	42 27 tremulous.	38	33½
7½	29·25	do.	42 21	39	34½
8	..	do.	42 19	39½	35
8½	..	N.W.	42 16	41	36
9	..	..	42 14	43	36½
9½	..	..	42 16 trem.	43	38
10	29·33	..	42 15 trem.	43	39
10½	..	..	42 15 trem.	43	39
11	..	..	42 8 trem.	45	39½
11½	..	..	42 6 trem.	44½	40
12	29·40	..	42 12 trem.	44½	42½
12½	..	..	42 12 trem.	44	40
13	..	..	42 12	45	41
13½	..	..	42 12	44½	40
14	..	..	42 13	44	42½
14½	..	..	42 12	45	40
15	..	..	42 11	44	40
15½	29·45	N.W.	42 14	44	39½
16	..	do.	42 14	44	39½
16½	..	..	42 14	44	39
17	..	..	42 11	44	40
17½	..	..	42 19	42	36½
18	..	..	42 24	41	36
18½	29·52	N.W.	42 28	40	35

The morning and evening mean extremes are 42·25 and 42·17 hygr. respectively. Hence the mean angle is 42·21, and the −7° difference of temperature only 4½°. N. B. The mean angle of all the observations in 1821 and 1822 is 42·11.

Observations made at Rumbles Moor, June 2, 1821.

Wind E.N.E.

Time.	Bolton Abbey. ′ ″	Pendle Hill.	Ingle-borough.	Gt. Whernside.	Therm. at Sum.	Base.
11 5	101 50 depr.	..	..	..	60	63½
11 30	..	..	..	25 40 elev.	60½	64
11 40	..	8 45 elev.	..	..	61	
12 0	101 42	..	..	..	62	65
12 10	..	..	..	25 57	59	
12 25	..	8 50	..	..	61	

Time.

Time.	Bolton Abbey. ′ ″	Pendle Hill.	Ingleborough.	Gt. Whernside.	Therm. at Sum.	Base.
12 35	..	..	11 18 elev.	..	62	64½
13 0	101 43	..	..	..	59	
13 10	..	..	..	26 0	61	67
13 20	..	..	11 21	..	60	
13 30	..	8 54	..	..	60	64½
14 30	101 46	..	..	..	59	63½
14 35	..	..	..	25 56	59½	
14 40	..	..	11 38	..	59	
14 45	..	8 50	..	..	59	64
15 50	101 52	..	..	..	59½	63½
16 0	..	..	..	26 0	59	63
16 5	..	..	11 31	..	56	
16 15	..	8 58	..	..	56	61½
17 7	101 34	..	..	..	56	61
17 10	..	..	..	26 9	55	
17 20	..	..	11 36	..	54	
17 25	..	9 12	..	..	54	59½
18 5	100 44	..	..	..	51½	57½
18 10	..	..	..	26 9	51	
18 15	..	..	12 0	..	50½	
18 25	..	9 24	..	..	50½	57
19 0	100 42 dep.	..	..	..	49	55½
19 10	..	..	..	26 35	49	
19 15	..	..	12 12 elev.	..	49	
19 20	..	9 20 elev.	..	..	48	54½
	Mean 101 17	Mean 9 5	Mean 11 45	Mean 26 7		
	Const. angle 101 14	C. an. 8 56	C. an. 11 33	C. an. 25 58		

Hygr. at
17 30
−9½

Rumbles Moor, June 19.

h. m.		′ ″		Pendle.
11 50	Pendle Hill elev.	8 54	48½	54¼
12 20	..	9 1	48½	53½
15 15	..	8 56	48	49
15 40	..	8 55	49½	49½
17 0	..	8 53	50½	48
17 45	..	8 55	49	46½
18 0	..	8 54	48½	47
18 45	..	8 55	47	46½
19 20	..	8 58	46½	46

Pendle is 500 feet higher than Rumbles Moor, and nearly west of it. An east wind produced fine weather at the former place, and the reverse at the latter; which may account for the thermometer

thermometer being on an average of 29 observations from $10^h\ 0^m$ to $19^h\ 20^m$; $1\frac{1}{2}°$ highest at Pendle Hill.

Barometer at	10· 0	28·911	Hygr.	−5°
Do.	13· 0	28·856		−4°
Do.	18·20	28·830		−6°

Beamsley Rock, June 14.

h.	m.		′	″	Therm.	Do. at Bolton.
12	0	Pendle Hill elev.	9	31	59	58
12	40	..	9	26	62	59
14	10	..	9	27	58½	58
15	15	..	9	8	54½	57½
17	10	..	9	10	53	56½
18	10	..	9	15	52½	55½

The thermometer at Bolton was 913 feet lower than on the Rock, at 14·35 the hygrometer was −14°. The barometer remained stationary at 28·925.

Beamsley Rock, June 27.

h.	m.		′	″	Therm.
16	35	Pendle Hill elev.	8	56	55½
17	20	..	9	0	55
18	15	..	9	7	52
19	35	..	8	54	47
19	50	..	8	54	46

At 17 20 the hygrometer was −10°. Barometer about 28·80. 9′ 3″ will be the apparent angle which will best agree with other observations.

Symon Seat, August 29.

h.	m.		′	″		Therm.
15	45	Gr. Almias Cliff, depr.	46	58	Wind S.E.	53
16	40	Do. ..	47	1	violent	52
		September 8.				
14	10	Gt. Almias Cliff,	47	29	S.W.	56

Observations of *other* stations made on both days agreed within a few seconds. The mean of the above *three* angles is the nearest to the true one.

The mean refraction in terms of the arc is ascertained by reciprocal observations of the angles of elevation and depression. The observations should however be made at the same instants of absolute time, and during the existence of the diurnal variation, it is almost superfluous to add that they must include the extremes, otherwise the greatest errors may be committed. The instruments should moreover be free from any *constant* error (such as those of collimation, &c.) or the refraction will be no longer correctly obtained in terms of the arc. When the cylindrical

drical rings of the telescope are not alike, and the one near the object glass proves to be of the largest diameter, the zenith distances will be all in excess, and the mean refraction (granting it to be one-twelfth) will appear negative until the arc becomes of such extent that one-twelfth thereof equals the error of the instrument. Still with numerous observations on arcs of various lengths the refraction (and of course the error of the instrument) may be discovered by ascertaining what constant correction must be applied to best reconcile the discordancies, and give the most uniform result.

When corresponding observations are made at both stations, the error of the instrument, although it vitiates the true value of the refraction, does not prevent the determination of the proper angle for calculation as accurately as could have been done with a perfect instrument. The refraction will come out too small, but then it will be applied to angles of elevation as much in defect*.

From the following statement it would appear that the Sector gave the elevations too little by 25″.

	Arc ′	″			Corrected
Jack Hill and Great Almias	3	11	$\frac{1}{13}$	neg.	$\frac{1}{17}$
Beamsley rock and Rumbles Moor	4	3	$\frac{1}{97}$	neg.	$\frac{1}{11}$
Beamsley rock and Symon hut ..	4	3	$\frac{1}{60}$	neg.	$\frac{1}{11}$
Rumbles Moor and Chevin	4	36	$\frac{1}{30}$	neg.	$\frac{1}{14.5}$
Beamsley rock and Jack Hill ..	6	4	$\frac{1}{90}$	neg.	$\frac{1}{17}$
Rumbles Moor and Jack Hill ..	6	18	$\frac{1}{47}$	pos.	$\frac{1}{12}$
Beamsley rock and Chevin	6	58	$\frac{1}{28}$	—	$\frac{1}{16.5}$
Rumbles Moor and Symon Seat ..	8	6	$\frac{1}{30}$	—	$\frac{1}{12}$
Rumbles Moor and Great Almias	8	30	$\frac{1}{128}$	—	$\frac{1}{18}$
Symon Seat and Jack Hill	8	34	$\frac{1}{4}$	—	$\frac{1}{13.5}$
Symon Seat and Great Whernside	8	40	$\frac{1}{50}$	—	$\frac{1}{14.5}$
Great Almias and Beamsley rock	9	8	$\frac{1}{274}$	—	$\frac{1}{20}$
Rumbles Moor and Observatory ..	11	1	$\frac{1}{44}$	—	$\frac{1}{16.5}$
Symon Seat and Great Almias ..	11	44	$\frac{1}{34}$	—	$\frac{1}{17.5}$
Great Whernside and Beamsley rock	12	40	$\frac{1}{30}$	—	$\frac{1}{15}$
Great Whernside and Rumbles Moor	16	37	$\frac{1}{21}$	—	$\frac{1}{14}$
Pendle and Rumbles Moor	16	46	$\frac{1}{30}$	—	$\frac{1}{17}$
Pendle and Symon Seat	17	48	$\frac{1}{23}$	—	$\frac{1}{15}$
Pendle and Great Whernside ..	20	26	[illegible]	—	$\frac{1}{13.5}$

By placing a delicate spirit level upon the cylindrical rings of the telescope, the Sector being well adjusted, and supposed to be *level*, it was found that the one near the object glass was higher by 35″. With this datum, together with the angular opening of the Ys and the diameter of the rings, the error of the instrument was calculated to be 28½″.

* When the power of the telescope is but small, will not the depressions be observed in excess?

A spirit

A spirit level firmly fastened to the upper, and another to the under surface of a firm brass bar being substituted for the former level of the large Sector, the mean of the readings gave 30″ as the error of the lesser instrument. A plane piece of glass with a very delicate mark in the middle superseded the cross wires, and being fixed upon the object with the index at zero, the bubble of the upper glass tube was adjusted to its mark. The telescope was next *inverted*, replaced upon the object, and the index levelled by the other glass tube, now uppermost.

The *double* of the angle was thus obtained, and the whole operation repeated, with the bar carrying the levels *reversed.* One fourth of the two double angles is of course the correct one.

Finally, the eye tube and the one containing the object glass were taken out of the smaller Sector, and reversed. The elevations were in consequence increased 52″, half of which, or 26″, is the error thereof. This is perhaps the most satisfactory test of the three, the other methods not being perfectly unobjectionable.

With this correction the mean refraction will be found to be about $\frac{1}{14 \cdot 5}$. The following remarks may render the more marked deviations from this quantity more intelligible.

1st. When the arc is but small, an error of a few seconds in the observations, or in the reduction of the height of the instrument to the ground, will cause a material alteration in the determined value of the refraction.

2nd. Some few of the angles at Rumbles Moor were only taken at the time of the diurnal variation, and the extremes were not always observed. Rumbles Moor and Jack Hill come under this class.

3d. The refractions at Great Almias are unusually small, but the station is on a group of huge rocks, which were probably heated to such an excess at the time of the observations by the previous intolerable heat of the sun's rays, as to render the lower strata of the air rarer than those immediately above.

4thly. Stations on isothermal curves will have refractions differing from those at right angles to them.

Excluding the journal kept at the Observatory, the mean of the heights of the barometer at the different stations would be 28·50, and the temperature 54. According to the below observations, the thermometer falls one degree for every ascent of 224 feet.

With these data, the *computed* will not be found to exceed the *observed* refraction very materially.

Remarks. The greatest difference of temperature was observed when the thermometer was 10 degrees lower at the Moor than at the Observatory. It was but very rarely that the air proved warmer at the more elevated station.

The

The thermometer on the mountain, however carefully shaded, is more suddenly and materially affected by the sun than the one at its base. On the approach of a shower, it will, for instance, suddenly fall several degrees.

When the thermometers differ but trivially, rain is generally the consequence.

Rumbles Moor and the Observatory are not upon the same isothermal curve; the mean temperatures of the latter will consequently require a small reduction.

M. temp. of				Near Ilkley.		at Cowper Cross.
3 obs. at 10^h in		Dec. 1820.	..	31°·3		5°·0 lowr
26	..	Jan. 1821	..	38·6		3·8
20	..	Feb.	..	37·1		3·7
23	..	Mar.	..	41·7		4·4
15	..	April	..	50·2		4·2
17	..	May	..	51·1		3·3
13	..	June	..	55·6		3·4
7	..	July	..	59·4		4·4
7	..	Aug.	..	62·7		4·4
2	..	Sep.	..	65·5		4·5
4	..	Dec.	..	47.0		4·6
11 obs. at	14^h	0^m to 18^h 45^m	Apr. 9	53·6		5·1
7	9	30 — 12 30	Apr. 17	49·4		3·7
13	10	30 — 12 30	July 25	60·5	(rain)	2.4

Cowper Cross is about three quarters of a mile W.N.W. of the station on Rumbles Moor, and is 1250 feet high Ilkley is two miles north of the Moor, and 296 feet high. If we exclude the last set of observations, the mean difference of the temperatures will be about 4°, and that of the altitudes 954 feet, which is equal to an ascent of 240 feet for a diminution of temperature of 1°.

Mean temp. of		1821.	Near Ilkley		At Rumbles Moor.
10 obs.	10·45 to 13·0	Apr. 25	66·9	..	3·2 lowr
12	8·45 — 14·0	— 30	52·5	..	5·3
18	11·0 — 19·30	June 2	62·0	..	5·0
7	11·15 — 12·45	July 13	66·9	..	4·0
9	10·15 — 12·15	— 21	62.6	..	3·7
10	10·30 — 12·0	— 27	61·9	..	4·4
13	11·0 — 13·0	— 28	60·3	..	5·7
15	11·30 — 15·0	Aug. 7	63·0	..	6·2

The station at Rumbles Moor being 1029 feet higher than the one near Ilkley, the ascent appears to be about 210 feet for a fall of 1° of the thermometer.

Mean

Mean temp. of			At the Obs.		At Cowper Cross.
4 obs. at 10·0 in		Dec. 1820	32·0	..	4°·8 lowr
26	..	Jan. 1821	39·4	..	4·6
20	..	Feb.	36·9	..	3·5
20	..	Mar.	44·3	..	4·7
5	..	April	45·3	..	4·2

The observatory is 851 feet lower than the Cross, and bears E. S.E. from it with a distance of 13½ miles. The ascent for the degree of the thermometer is equal to 198 feet.

M. temp. of		1821.	A the Obs.	At Rumbles Moor.
5 obs. at	11.45 to 13.15	Mar. 13	52·5	6·5 lowr
15	10. 0 to 18. 0	— 30	47·5	7·0
22	7.30 to 18.30	Apr. 5	45·0	4·6
6	7.30 to 10. 0	— 12	43·8	4·8
9	6. 0 to 10. 0	— 19	38·6 (mist & rain)	2·1

The observatory is 67,082 feet E.S.E of Rumbles Moor, and 926 feet lower. Hence 185 feet for every fall of 1° of temperature.

Mean temp. of 13 obs. at 10·30 to 12·30, July 30; at Ilkley Wells 57·2; at Rumbles Moor 2·8 lower. Rumbles Moor is 637 feet above the Wells.—Ascent required 224 feet.

Mean temp. of 4 obs. at 13.15 to 14.0, Oct. 29, near Ilkley, 58.6; Beamsley Rock, 3.0 lower.

Difference of altitude 1021 feet.

Mean temp. of 12 obs. at 12.30 to 18.0, June 14, near Bolton, 57.5; Beamsley Rock, 1.7 lower.

Difference of altitude 913 feet.

Mean temp. of 2 obs. Aug. 20 and Aug. 31, at Kettlewell, 59; at Great Whernside, 7.5 lower.

Difference of altitude 1573 feet (by barometer).

Mean temp. of 4 obs. Sept. 11, 13.22 and 24, at Downham, 58.5; at Pendle 9.7 lower.

Difference of altitude 1352 feet (by barometer).

Comparison of the Angles given by Ramsden's great Theodolite, and the Horizon Sector.

The angles are reduced to the ground at Rumbles Moor.

Boulsworth, elev...	..	13.29 Theod.	13.29 Sector.
Pendle Hill, bn. elev.	..	9.16	9.18
Great Whernside, elev.		25.45	26. 8

As all the angles by the Sector include its error of 26″, it would appear that the Theodolite was affected by a similar defect, or that the observations were made during the heat of the day, when the variation of refraction was in force. That the elevation of Great Whernside was incorrectly observed will be clearly proved in its proper place.

XXXVII. *Pro-*

XXXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Dec. 6, 1821.—A PAPER communicated by the Society for the Improvement of Animal Chemistry was read, entitled "On some Alvine Concretions found in the Colon of a young man in Lancashire after death." By John George Children, esq. F.R.S.

Dec. 13. A paper was read "On the Concentric Adjustment of a triple Object Glass." By W. H. Wollaston, M.D. and V.P.R.S.

Also a paper entitled, "On a new Species of Rhinoceros, found in the Interior of Africa; the skull of which bears a close resemblance to that found in a fossil state in Siberia and other countries." By Sir Everard Home, bart. V.P R.S.

Dec. 20. There was read a paper on the Electrical Phenomena exhibited *in vacuo*, by Sir Humphry Davy, bart. P.R.S.

Jan. 10, 1822. An extract of a letter from Capt. Basil Hall, R.N. to Dr. W. H. Wollaston, containing Observations on a Comet seen at Valparaiso, was read.—Also

Elements of Capt. Hall's Comet, in a letter from Dr. Brinkley to Dr. Wollaston.

Jan. 17. A paper on the Ultimate Atoms of the Atmosphere, by Dr. W. H. Wollaston, was read.—Also

A paper on the Expansion in a Series of the Attraction of a Spheroid, by James Ivory, esq.

Jan. 24. Two papers were read. 1. On the late Depression of the Barometer, by Luke Howard, esq. 2. On the Anomalous Magnetic Attraction of Hot Iron, by P. Barlow, esq.

ASTRONOMICAL SOCIETY OF LONDON.

Feb. 8.—The second annual general meeting of this Society was held this day; when a Report was read on the State of the Society and its finances, which appeared to be in a very flourishing condition. The first volume of their *Memoirs* is in the press, and will shortly be published.

The following is a list of the Officers, which were chosen for the ensuing year: viz.

President.

Sir William Herschel, LL.D. F.R.S.

Vice-Presidents.

Major T. Colby, *Roy. Eng.* LL.D. F.R.S. L. & E.
Sir H. C. Englefield, Bart. F.R.S. L. & E. F.S.A. & L.S.
Davies Gilbert, Esq. V.P.R.S. & F.L.S.
D. Moore, Esq. F.R.S. S.A. & L.S.

Treasurer.

Rev. W. Pearson, LL.D. F.R.S.

Secre-

Secretaries.

C. Babbage, Esq. M.A. F.R.S. L. & E.
F. Baily, Esq. F.R.S. & L.S.
J. F. W. Herschel, Esq. M.A. F.R.S. L. & E. (*Foreign.*)

Council.

G. Birkbeck, M.D.
B. Gompertz, Esq. F.R.S.
O. G. Gregory, LL.D.
S. Groombridge, Esq. F R.S.
J. Horsburgh, Esq. F.R.S.
Maj. Gen. John Rowley, *Roy. Eng.* F.R.S.
J. South, Esq. F.R.S. & L.S.
E. Troughton, Esq. F.R.S. L. & E.

The Names, under each Office, are arranged alphabetically.

ACADEMICAL SOCIETY OF THE LOWER LOIRE.

This society has proposed a prize consisting of a gold medal value 300 francs, for the best answer to questions respecting the yellow fever. It is required to trace its origin, to specify its causes and nature; to describe the state of the atmosphere and local circumstances where it prevails; to notify its identity or otherwise with similar fevers in Europe, &c.; to distinguish whether it be complicated with any other malady. There is also a second subject relating to the means for preventing its spreading, the proper modes of quarantine, &c. The memoirs to be sent, post free, to the secretary of the society before the 1st of May 1822. Each to bear a motto with a repetition in a sealed paper, containing, as usual, the author's name and address.

ROYAL SOCIETY OF MEDICINE AT MARSEILLES.

This society has proposed the following questions: 1. To determine the structure and functions of the spinal marrow. 2. To describe the nature, causes, symptoms, and treatment of the diseases by which the spinal marrow is affected. It is desired that clinical observations and pathological anatomy should be made the principal objects of the memoirs. They may be written in Latin or French. The extent of time allowed is till July 1822, and the prize a gold medal.

SOCIETY OF SCIENCES AND ARTS AT METZ.

When the nozzle of a blowing machine is placed at a certain distance from that of the tuyère, a stronger current of air is obtained than when both are placed together, as is frequently done. This effect is produced by various causes dependent on the elastic nature of the fluid in motion, and of the surrounding atmosphere. The Society of Sciences and Arts at Metz have founded the following prize questi[illegible] this experiment: "What are the changes necessary to b[illegible]e in the tuyère of blowing machines, to

to introduce, in the most advantageous manner, the good effect indicated above, or any other improvement for the rapid transmission of air to greater or smaller distances." The prize is 300 francs, and is to be adjudged in April 1822.

ROYAL ACADEMY OF SCIENCES OF TOULOUSE.

This Academy has proposed as the subject of prize essays, "a physico-mathematical theory of drawing and forcing pumps, stating the ratio between the moving power and the quantity of water elevated; attention being given to all the obstacles which the force has to overcome." Among these obstacles are enumerated, the weight and inertia of the column of water, its friction against the tubes, its contraction at the apertures of the valves, the weight and friction of the pistons, the weight of the valves, the inequality between the upper and lower surface at the moment the pressure opens them, &c. The papers are to be written in French or Latin, and sent in before May 1823. The prize is a gold medal of 500 francs value.

XXXVIII. *Intelligence and Miscellaneous Articles.*

A NEW GREEN COLOUR.

To Dr. Tilloch.

SIR,—THE discovery of a new green colour by M Bizio, which was announced in your last Number, has induced me to make some experiments with other vegetable substances; and I beg to state that I have succeeded in producing a green colour brighter than what I could procure by using coffee, and possessing chemical properties somewhat different. I formed a strong decoction of tobacco by boiling it for some time in pure water; then added solution of sulphate of copper, and precipitated with subcarbonate of potassa. The precipitate when dry is of a light green colour. Mixed with linseed oil it became darker and brighter, and very like a rich grass green. Dissolved in nitric acid it forms a green solution. It also tinges sulphuric acid of a green colour. I do not find that it is acted upon either by water, alcohol or ether.

I am, sir, your obedient servant,

No. 6, Dartmouth-street, Westminster, Feb. 18, 1822. CHARLES M. WILLICH.

Mr. Willich with the above communication favoured me with specimens of his new green, both dry and mixed up with linseed oil. It is a most beautiful colour, and will probably prove highly useful in the arts.—A. T.

CARBONATE OF LIME.

Mr. Dalton, in a paper on the analysis of spring and mineral waters, states, "that all spring water containing carbonate or super-carbonate of lime is essentially limy or alkaline, by the colour tests. And this alkalinity is not destroyed till some more powerful acid, such as the sulphuric or muriatic, is added, sufficient to saturate the whole of the lime. Indeed, these acids may be considered as sufficient for tests of the quantity of lime in such waters; and nothing more is required than to mark the quantity of acid necessary to neutralize the lime. It does not signify whether the water is boiled or unboiled, nor whether it contains sulphate of lime along with the carbonate; it is still limy in proportion to the quantity of carbonate of lime it contains. Agreeably to this idea, too, I find that the metallic oxides, as those of iron or copper, are thrown down by common spring water, just the same as by free lime. Notwithstanding, this carbonate of lime, in solution in water, contains twice the acid that chalk or limestone does. I fully expected the super-carbonate of lime in solution to be acid; but it is strongly alkaline, and scarcely any quantity of carbonic acid water put to it will overcome this alkalinity. Pure carbonic acid water is, however, acid to the tests. I could not be convinced of the remarkable fact stated in this paragraph, till I actually formed super-carbonate of lime, by supersaturating lime water in the usual way, till the liquid from being milky became clear. It still continued limy, and was even doubtfully so when two or three times the quantity of acid was added. It should seem, then, to be as impossible to obtain a neutral carbonate of lime, as it is to obtain a neutral carbonate of ammonia, in the sense here attached to the word neutral."—*Memoirs of the Manchester Society.*

ANALYSIS OF TEA.

In the 24th number of the Quarterly Journal of Science, Mr. Brande has published analyses of black and of green tea, from which he finds that "the quantity of astringent matter precipitable by gelatine is somewhat greater in green than in black tea, though the excess is by no means so great as the comparative flavours of the two would lead one to expect. It also appears that the entire quantity of soluble matter is greater in green than in black tea, and that the proportion of extractive matter not precipitable by gelatine is greatest in the latter."

"Sulphuric, muriatic, and acetic acids, but especially the first, occasion precipitates in infusions both of black and green tea, which have the properties of combinations of those acids with tan. Both infusions also yield, as might be expected, abundant black precipitates, with solutions of iron; and when mixed with acetate,

acetate, or more especially with subacetate of lead, a bulky buff-coloured matter is separated, leaving the remaining fluid entirely tasteless and colourless. This precipitate was diffused through water, and decomposed by sulphuretted hydrogen; it afforded a solution of tan and extract, but not any traces of any peculiar principle to which certain medical effects of tea, especially of green tea, could be attributed."

Mr. Brande observes, that there is one property of strong infusions of tea, belonging especially to black and green, which seems to announce the presence of a distinct vegetable principle; namely, that they deposit, as they cool, a brown pulverulent precipitate, which passes through ordinary filters, and can only be collected by deposition and decantation; this precipitate is very slightly soluble in cold water of the temperature of from 50° downwards, but it dissolves with the utmost facility in water of 100° and upwards, forming a pale-brown transparent liquid, which furnished abundant precipitate in solutions of isinglass, of sulphate of iron, of muriate of tin, and of acetate of lead; whence it may be inferred to consist of tannin, gallic acid, and extractive matter.

The following table is given by Mr. Brande as showing the respective quantities of soluble matter in water and alcohol, the weight of the precipitate by isinglass, and the proportion of inert woody fibre on green and black tea of various prices:

One hundred parts of tea.	Soluble in water.	Soluble in alcohol.	Precipitate with jelly.	Inert residue.
Green hyson, 14*s*. per lb. . .	41	44	31	56
Ditto, 12*s*.	34	43	29	57
Ditto, 10*s*.	36	43	26	57
Ditto, 8*s*.	36	42	25	58
Ditto, 7*s*.	31	41	24	59
Black souchong, 12*s*.	35	36	28	64
Ditto, 10*s*.	34	37	28	63
Ditto, 7*s*.	36	35	24	64
Ditto, 6*s*.	35	31	23	65

PREPARATION OF QUININE.

M. J. Voreton, of Grenoble, employs the following method in preparing Quinine, by which he says he is enabled to procure about two ounces and a half of Quinine from eleven pounds of Cinchona, instead of an ounce and a half, or an ounce and three quarters procured by the common process. The Cinchona reduced to a coarse powder is to be digested in water, acidulated

with

with about one hundredth of its weight of muriatic acid. At the expiration of 24 hours, the Cinchona is to be strongly pressed, to be again treated with dilute muriatic acid, and the processes are to be repeated till the Cinchona loses its bitterness. The filtered infusions are to be mixed and treated with excess of pure magnesia, the mixture to be boiled for a short time and then suffered to cool. The magnesian precipitate is to be washed with cold water, dried, and digested in alcohol: by distilling this solution the Quinine is obtained.—(*Annales de Chimie.*)

EXPLOSION OF CHLORINE AND HYDROGEN.

It has been long known that a mixture of chlorine and hydrogen explodes when exposed to the direct action of the sun's rays. In order to try if this effect could be produced by the radiation of a common culinary fire, Professor Silliman filled a common Florence oil-flask (well cleaned) half full of chlorine gas, and was in the act of introducing the hydrogen in the pneumatic cistern. "There was not only no *direct* emanation from the sun, but even the *diffuse* light was rendered much feebler than common by a thick snow-storm, which had covered the skylight above with a thick mantle, and veiled the heavens in a singular degree for such a storm. Under these circumstances, the hydrogen was scarcely all introduced before the flask exploded with a distinct flame; portions of the glass stuck in the wood work of the ceiling of the room, and the face and eyes escaped by being out of the direction of the explosion; nothing but the neck of the flask remained in hand. This occurrence then proves, that a mixture of chlorine and hydrogen gas may explode spontaneously in a diffuse light, and even in a very dim light.—*American Journal of Science, Vol.* 3. *No.* 2. *p.* 343.)

LINEN TRADE.

We understand that a very great improvement in the method of bleaching linen and yarn has lately been made by Mr. Crookshank of Dublin.—As far as we have been able to ascertain, its chief merit consists in the disengaging the chlorine from the oxymuriate of lime—by which ingenious process it is enabled to act with full force upon the cloth and yarn. Independently of a considerable saving in the quantity of bleaching liquor, by which the possibility of injuring the linen is prevented, this process combines some other very important advantages. It has already been tried on a considerable scale, and has met with the full approbation of a gentleman of chemical celebrity.—We are informed that Mr. Crookshank has submitted his discovery to the Linen Board, and proposed to exhibit its advantages by a course of experiments. We hope, therefore, that the process will shortly be made public, for the benefit of the trade.—*Dublin Newspaper, 4th Feb.*

" *Example of* 'PERSONAL ABUSE,' *in a late Discussion.*"

From a Correspondent.

1. " The computations have been conducted by the assistance of Mr. Ivory's *most masterly* investigations of the attractions of spheroids. 2 Jan. 1820."—*Journ. R. I.*

2. " Entertaining, as I unfeignedly do, the profoundest respect for the analytical talents of Mr. Ivory, and admitting most readily, that he has contributed, *more than any person now living*, to advance the reputation of this country among our contemporaries abroad, with regard to abstract mathematics. 31 Dec. 1820."—*Journ. R. I.*

3. " I have some concessions to make to Mr. Ivory, and the computations of such a mathematician as he is, are not to be hastily or lightly examined. 3 Nov. 1821."—*Phil. Mag.*

4. " A mathematician of Mr. Ivory's acknowledged celebrity and transcendent attainments. Dec. 1821."—*Journ. R. I.*

5. To these passages may be added a fifth quotation, not wholly inapplicable to the merits of the present case. " It seems, indeed, as if mathematical learning were the *euthanasia* of physical talent; and unless Great Britain can succeed in stemming the torrent, and checking the useless accumulation of weighty materials, the fabric of science will sink, in a few ages, under its own insupportable bulk. A SPLENDID EXAMPLE has already been displayed by the Author of the article *Attraction* in this Supplement: and, to do justice to our neighbours, it must be allowed that they have received the boon with due gratitude, and acknowledged it by merited applause: " all the analytical difficulties of the problem," say Legendre and Delambre (*Mém. Inst.* 1812) " vanish at once before this method: and a theory, which before required the most abstruse analysis, may now be explained, in its whole extent, by considerations perfectly elementary." It is, in fact, only when a subject is so simplified, that the investigation can be considered as complete, since we are never so sure that we understand the process of nature, as when we can trace at once in our minds all the steps by which that process is conducted."— *Biography of Lagrange, Apr.* 1821. *Suppl. Enc. Brit.* 1822. V. 199.

These passages, which, there is reason to think, are the production of the same pen, are the *only personalities* that I have been able to discover, relating to Mr. Ivory, in the writings of their author: the mentioning his opinions with *levity*, so far only as they are asserted to be unfounded, does not appear to me to constitute a *personality;* much less to deserve the epithet of *personal abuse.*

London, 5 Jan. 1822. A. B. C. D.

PRESERVING OBJECTS OF NATURAL HISTORY.

M. Drapier, Professor of Chemistry and Natural History, and one of the Editors of the *Annales Generales des Sciences Physiques,"* has substituted with success, in lieu of the poisonous matters employed in preserving objects of natural history, a soap composed of potash and fish oil. He dissolves one part of caustic potash in water, and adds to the solution one part of fish oil: he rubs the mixture till it acquires a pretty firm consistence. When it is completely dry, he reduces it to powder with a rasp. One part of this powder is employed in forming a soft paste or liquid soap, by means of an equal quantity of a solution of camphor in musked alcohol. This liquid soap is well rubbed upon the skin of the bird, previously cleared of its fat, and the other part of the soap and powder is plentifully scattered between the feathers. Thus prepared, the bird is placed in a moist situation, in order that the particles of soap may soften and attach themselves perfectly to the feathers, the down, and the skin. It afterwards is put in a dry place. By this means it completely resists the attacks of larvæ, and has neither the danger nor the inconvenience of arsenical preparations, which, as is well known, stain and spoil the extremities of the feathers and down.

TO PREPARE OIL PROPER TO BE APPLIED TO WATCH-WORK AND OTHER DELICATE MACHINERY.

The oil best adapted for diminishing friction in delicate machinery should be free from all acid and mucilage, and be capable of enduring intense cold without freezing. The oil, in one word, should be pure *ealin* free from even a trace of *stearin.*

It is by no means difficult to extract the ealin from any of the fine oils and even from fats, by following M. Chevreul's process, which consists in treating the oil in a matrass, with seven or eight times its weight of alcohol nearly boiling, decanting the liquid and suffering it to cool. The stearin separates in the form of a crystalline precipitate. The alcoholic solution is then to be evaporated to one-fifth of its volume, and the ealin will be obtained; which should be colourless and tasteless, almost free from smell, without action on infusion of litmus, having the consistence of white olive oil, and not easily congealable.

VACCINATION.

Dr. Thompson, of Edinburgh, has started a new theory of vaccination, viz. that it is not a certain preventive of small-pox, but that it is a better preservative than the small-pox itself. The Doctor is of opinion, that what is denominated chicken-pox is a true variolous disease, modified by previous small-pox or cow-pox, and that the chicken-pox is the mildest after vaccination has been undergone.

A KNIFE FOUND IN THE HEART OF A TREE.

Some sawyers of the name of Short were employed to saw a fir-tree raised from a turf bog, or peat moss, as it is elsewhere called. The tree was dug up six feet below the surface, in the Rev. Mr. Steward's property, in Tyrone, and brought to his residence at Grange, near Armagh, where the Shorts were employed to saw it. They proceeded in their task, but having advanced about half way through the log, the saw was arrested. They then turned the log, and continued to saw it in the opposite direction, when they discovered the blade of a knife, in a hole in which a man's fist could lie. The conjecture of the sawyers was, that the knife had been stuck into the bark, and that the hole was occasioned by the rotting of the handle, as it was enveloped by the annual coating of the growing tree.

ASTRONOMY.

Mr. Schumacher, the Danish astronomer, has recently established an astronomical journal, which promises to be exceedingly interesting to the lovers of astronomy. It is printed in quarto, in separate sheets (like some of our Sunday newspapers); and is published as often as the matter accumulates to a sufficient quantity for a number. The first number appeared towards the latter end of last year; and already six numbers have been published: the seventh is now in the press. It is written in German, which will prevent its being much circulated in this country: but many of the articles are worthy of being translated, and distributed here.

The eclipses of Jupiter's satellites, as given in the Nautical Almanac for this year, are almost all of them erroneous. There is sometimes as much as 2′.10″ difference between the values given in that work, and the correct value.

The North polar distances of the principal stars, as given in the Nautical Almanac for 1824, are also erroneous, in consequence of a derangement in the mural circle at Greenwich. The particulars of the circumstances, attending this derangement, were communicated by Mr. Pond in a letter to the Royal Society as far back as November last: but none of the public journals have yet alluded to the subject, nor has any thing further transpired relative thereto: except that we are informed that a Committee of the Royal Society has been appointed to inspect the state of the instrument.

Major General Sir Thomas Brisbane, in his recent voyage to New South Wales, observed an occultation of *Regulus, at sea.* And, what is a remarkable circumstance, the star appeared on the disc of the moon (at its emersion) for *two minutes:* a longer time than has ever yet been recorded.

Mr.

Mr. Schumacher's *Astronomische Hülfstafeln* will not be ready for publication these two months: this work has been, in some measure, superseded by a similar publication of Mr. Baily's, in this country; we regret however that this latter work is printed for private circulation only, and not for general use.

The observers of double stars will be pleased to hear that there is a list of several which have been observed by Dr. Struve, at the new Observatory at Dorpat, in Mr. Bode's *Astronomische Jahrbuch* for 1824.

The Russians are about to measure an arc of the meridian in their country: Dr. Struve and M. Walbeck are to have the direction of the business.

LIST OF PATENTS FOR NEW INVENTIONS.

To John Hague, of Great Pearl-street, Spitalfields, Middlesex, engineer, for his improved method of making metallic pipes, tubes, or cylinders, by the application and arrangement in apparatus of certain machinery and mechanical powers.—Dated the 29th Jan. 1822.—6 months allowed to enrol specification.

To Sir William Congreve, of Cecil-street, Strand, Middlesex, baronet, for certain improved methods of multiplying fac-simile impressions to any extent.—29th Jan.—6 months.

To Peter Ewart, of Manchester, Lancashire, civil engineer, for his method of making coffer dams.—29th Jan.—2 months.

To Robert Bill, of Newman-street, in the parish of St. Mary-le-bone, Middlesex, gentleman, for his improved method of manufacturing metallic tubes, cylinders, cones, or of other forms, adapted to the construction, and for the construction of the masts, yards, booms, bowsprits, or casks, or for any other purposes to which they may be applicable.—5th Feb.—6 months.

To Frederick Louis Tatton, of New Bond-street, Middlesex, watch-maker, who, in consequence of discoveries by himself and communications made to him by a certain foreigner residing abroad, is in possession of an astronomical instrument or watch by which the time of the day, the progress of the celestial bodies, as well as carriages, horses, or animals, may be correctly ascertained.—9th Feb.—2 months.

To George Holworthy Palmer, of the Royal Mint, engineer, for certain improvements in the production of heat by the application of well-known principles not hitherto made use of in the construction of furnaces of steam-engines and of air furnaces in general, whereby a considerable saving in the expenditure of fuel is obtained, and the total consumption of smoke may be effected.—12th Feb.—6 months.

To John Frederick Smith, of Dunston Hall, in the parish of Chesterfield, Derbyshire, esq., for his improvements in dressing of

of piece goods made from silk or worsted, or of both these materials.—12th Feb.—4 months.

To Sampson Davis, of Upper East Smithfield, Middlesex, gun-lock maker, for his improvement upon the lock for guns and other fire-arms, which enables the same lock to be used upon the percussion principle, or with gunpowder without charging the lock or hammer.—12th Feb.—2 months.

To Thomas Brunton, of the Commercial Road, Middlesex, chain cable and anchor manufacturer, for his improvement upon the anchor which he conceives will be of public utility.—12th Feb.—6 months.

To Elisha Peck, of Liverpool, Lancashire, merchant, who in consequence of a communication made to him by Ralph Bulkley, a foreigner resident in the city of New-York, and a citizen of the United States of America, is in possession of an invention of a certain machinery to be worked by water applicable to the moving of mills and other machinery of various descriptions for the forcing or pumping of water.—Feb.—6 months.

METEOROLOGY.

To Dr. Tilloch.

Hartwell, February 19, 1822.

Sir,—There are some circumstances so remarkable in the present season, that I have deemed them worthy of being noted down in your magazine, with a view that they may be compared with the observations of meteorologists in different parts of the country. That the winter has been very unusually mild, and that there has been a considerable proportion of wet and blowing weather, must have struck every body; but on a minute inspection of the instruments of meteorology, I find peculiarities which are less obvious to common observation. In four days out of seven (on *an average*) in every week since the 1st of last December, the temperature has risen more than three degrees between nine o'clock at night and midnight; there has been a constant fluctuation of temperature, as well as of barometrical pressure, all the winter, with the exception of a few weeks of late; but the above circumstance seems to show that the changes from a lower to a higher temperature have usually taken place between nine o'clock and midnight; for if the weather had not changed at that time, the heat would have gone on declining through the night, as usual. Should any of your correspondents be desirous of it, I can send you minutes of the observations taken from my journal. Of the cause of the above phenomena I am ignorant; but if electricity be principally concerned in producing atmospherical changes, its irregular distribution this season (which several other circumstances indicate) may perhaps account for the unusual

periods of the changes of temperature. The perpetual changes of the weather, accompanied with showers of rain in December and January, have induced a false belief that a real greater quantity of rain has fallen this season than is usual, which some astrologers have not been backward hastily to attribute to the conjunction of Jupiter and Saturn. In fact, there has been no very great increase in the quantity of rain fallen this winter. The extraordinary depression of the quicksilver in the barometer is another remarkable circumstance, as it has occurred more than once during the present season. On the night of the 24th December at eight o'clock it was as low as 27·97°, the thermometer being 46° of Fahrenheit. By twelve o'clock the barometer had risen about $\frac{1}{100}$ of an inch, and the thermometer had risen to 48°; and the wind, which was high, became stormy, and blew in violent gales intercepted by calms, and accompanied by torrents of rain.

The most violent gale we have had this year, took place at five in the morning of the 23d December; it blew tremendously for above an hour, and was followed by a dead calm; but the wind got up again and blew very *cold*. After sun-rise on the 25th, I have noticed that previously to all the heavy gales that have blown of late, there has been an elevation of temperature.

We have had but three frosty nights this year; but my house stands half way up a hill which rises from one of the valleys of the Medway, and the upper half of the hill shelters it from the north.

The contracted range of temperature in each day is another remarkable circumstance, the difference between the maximum and minimum being very inconsiderable during the unseasonably *mild* weather of the *winter solstice*; and it is curious, that a similar approximation of the maximum and minimum of temperature was observed during the very *cool* weather which happened about the last *summer solstice*. I merely hint at these peculiarities at present, without comment; as the grand cause of these phenomena of the weather is as yet unexplained, and requires the accurate observation of ages to develop it. For the present we must content ourselves with observations, and avoid as much as possible entangling them with hypothesis.—I have prepared for some future number, a copious list of plants which have flowered prematurely this winter, and which I hope to send you in a few weeks. At present the *Scilla Peruviana* flowers in the open ground:—this is the most remarkable of the premature productions of this warm winter, as this plant is in general very constant to its period, which is the beginning of May.

I remain, in haste, yours,

J. Forster.

Manchester,

Manchester, February 7, 1822.

SIR,—I have inclosed you my annual results of the weather for the past year, for insertion in your valuable magazine:

And am, sir, your most obedient servant,

To Dr. Tilloch. THOMAS HANSON.

METEOROLOGICAL RESULTS

Of the Atmospherical Pressure and Temperature, Rain, Wind, &c. deduced from Diurnal Observations made at Manchester, in the year 1821, by Mr. Thomas Hanson, Surgeon.

Latitude 53° 25′ North.—Longitude 2° 10′ West of London.

1821.	Barometrical Pressure							Temperature.					Rain.					Wind.										
	Mean.	Highest.	Lowest.	Range.	Great var. in 24 hrs.	Spaces in inches.	No. of changes.	Mean.	Highest.	Lowest.	Range.	Great. var. in 24 hrs.	Manchester. Inches.	Manchester. Wet Days.	Ardk. Inches.	L. mm. Inches.	Cp-ll. Inches.	North.	North East.	East.	South East.	South.	South West.	West.	North West.	Variable.	Brisk.	Boisterous.
Jan.	29·75	30·64	28·96	1·68	·66	3·10	4	40°·3	55°	29°	3[illegible]°	15°	1·095	6	1·3[illegible]	1·045	1·705	0	7	0	5	1	8	3	2	5	1	0
Feb.	30·11	30·52	29·20	1·32	·42	2·15	7	37·9	55	25	30	20	·535	4	·844	·260	·528	0	9	2	0	0	4	0	11	2	·	0
March	29·42	30·15	28·92	1·23	·67	5·20	11	44·7	61	29	32	36	2·625	18	3·145	2·947	3·870	0	3	0	0	0	17	0	3	2	1	0
April	29·51	30·00	28·96	1·04	·50	4·10	8	52·2	74	33	41	25	3·520	19	3·984	2·720	3·523	0	1	0	0	1	23	0	4	1	2	1
May	29·75	30·10	29·16	·94	·40	4·10	6	52·6	73	34	39	30	2·520	13	3·194	2·232	2·884	0	1	0	2	5	18	1	9	2	0	0
June	29·97	30·28	29·55	·73	·34	1·8	7	57·4	74	40	34	27	1·050	8	1·458	1·590	1·203	0	8	5	1	3	7	3	2	1	0	0
July	29·79	30·18	29·32	·86	·40	3·20	7	61·9	81	44	37	23	1·907	[illegible]	2·490	1·143	1·872	0	1	5	4	0	0	10	5	0	0	0
August	29·80	30·08	29·25	[illegible]	·50	2·80	8	63·1	79	48	31	23	3·135	17	3·519	2·357	3·281	0	0	0	3	2	12	10	3	1	0	0
Sept.	29·69	30·10	29·10	·94	·50	3·20	13	50·6	76	46	30	23	4·593	23	5·466	3·845	4·279	0	0	0	2	1	25	2	0	0	2	1
Oct.	29·71	30·16	28·75	1·41	·67	5·20	10	52·4	66	39	27	26	2·900	20	3·287	2·270	4·323	0	0	0	[illegible]	1	20	5	0	0	0	1
Nov.	29·59	30·16	28·92	1·24	·67	5·00	9	47·8	62	35	27	17	4·390	2[illegible]	5·40	3·771	3·442	0	1	0	5	0	16	5	2	1	1	2
Dec.	29·24	30·10	28·10	1·94	·50	7·80	15	43·8	62	31	31	23	3·800	19	4·97	3·880	3·264	0	7	0	6	0	14	0	1	3	1	2
	29·69	30·20	28·94	1·17	·5[illegible]	47·65	105	51·[illegible]	58	3[illegible]	3[illegible]	24	31·890	180	39·108	28·0[illegible]7	34·778	0	38	10	33	12	159	[illegible]	49	18	9	10

The annual mean temperature of the past year is fifty-one degrees; being about two degrees above the average: the mean of the first three months, 40°9; second 54°1; third, 61°9; fourth, 48°; of the six winter months, 44°4; six summer months, 57°9. The maximum, or hottest state of the year was 81°, which occurred on the memorable 19th of July, the Coronation of King George the Fourth; the minimum or coldest state was 23°, which is only 9 below freezing; this happened on the 4th January, making an annual variation of 58°. From the above, the reporter is enabled to draw the following comparison between the past and preceding year, viz. the average heat of the six summer months of 1821 was nearly one degree more than that of 1820, and the heat of the six winter months three degrees above the corresponding ones of the preceding year; so that the temperature of 1821 has been more mild than usual, and not marked by any very great extremes.

The annual mean elevation of the barometer is nearly twenty-nine inches and seven-tenths; highest 30·65, which was on the 23rd of January; lowest 28·16, which happened on the 28th of December: the difference of these extremes makes 2·49 inches; mean of the six summer months 29·75; of the six winter months 29·63. The mean daily movements of the barometrical surface measure nearly forty-eight inches: total number of changes one hundred and five. The barometer throughout the month of February was remarkably high and desultory in its movements: on the contrary, in the month of December it oscillated most extraordinarily; and towards the close of the year very low: the utmost depression was the minimum of the year.

Much has been said about the wetness of the past year. My annual account scarcely amounts to 32 inches in depth, which is certainly under the average for Manchester. Mr. John Blackwall of Crumpsall, makes his annual fall three inches more; and Mr. John Dalton, for Ardwick, nearly eight inches more than mine. On the contrary, Mr. Edward Stelfox of Lymm near Warrington, has only registered a fall of twenty-eight inches. The differences in our annual statements of rain from places so near together are singular, and certainly require an attentive inquiry. The only difference in our apparatus is, that Mr. Dalton's rain funnel is larger; mine, Mr. Blackwall's, and Mr. Stelfox's are made alike, the same size, and of one material, which is that of copper. Provided our calculations of the method of measuring the rain collected in these funnel-areas be correct, and which I have every reason to conclude is the case; and provided their surfaces are parallel with the horizon, and at sufficient distances from trees, buildings, or any object that might obstruct a free access, it must follow that there can be no error in our results. I have noted down

down 180 days on which rain fell more or less, which number is one less than last year. In the last five months of 1820 there were 85 wet days; the number in the corresponding ones of 1821 is 101. February was the dryest, and September and November the wettest.

The south, south-west, and west winds have been the most prevalent: those winds were noticed to blow on 224 days. On the 18th, 19th, and 20th of March, (about the vernal equinox) the wind blew hurricanes from the north-west, attended with rain, snow, and sleet. On the night of the 30th of November and following morning, the wind blew a most violent gale from the south-west accompanied with hail and rain; the damage done in consequence, by the falling of chimneys, unroofing of houses, &c. was great; several lives were lost in Liverpool and other places, and a large number of vessels suffered in the harbours and on the neighbouring coasts.

Bridge-street, 28th January, 1822.

BRILLIANT PHÆNOMENON.

[A brief notice of the following brilliant phænomenon appeared in several of the journals not long after its occurrence. We have been favoured with the following more particular account, written by an eye-witness.]

"On the night of the 2d of August 1819, in north latitude 5°, and west longitude 20°, a most astonishing degree of brilliancy was exhibited by the ocean, under circumstances which added in a very remarkable manner to the magnificence of the spectacle.

"Every appearance of an approaching storm was indicated; black clouds traversed the firmament in hurried confusion; the wind, veering from point to point, rushed by in short heavy blasts—suddenly it lulled, darkness became intense, and the most profound silence ensued. Anxiety was now excited to its utmost height; in breathless suspense we awaited the result of an impending storm, which threatened almost sure destruction to the stoutest vessel, should she encounter the first discharge of its fury. From this region of gloom, and horrors anticipated, our trusty bark emerged; and instantly, as by magic, we were ushered into an element flaming with resplendence. The ocean presented one continued sheet of illumination; the phosphorescent blaze, radiated from the blue-tinted wave, produced a variety of beautifully coloured light, which at intervals shone with the clearness of noon-day. The reflection falling on the sails and white masts gave a rich metallic cast: the bowsprit and yard, from their favourable situation and the fresh coat of paint recently put on them, attracted the highest degree of admiration; the silvery lustre which they displayed could scarcely have been

surpassed

surpassed by the original metal in its most polished state. So rapid and unexpected a transition from darkness impenetrable to a scene dazzling with splendour, produced an effect truly electric. Exclamations burst from the wondering spectators, and rung in echoes loud throughout the ship. Usurped by the novel and imposing character of the phænomenon, the mind seemed inaccessible to every other impression, and, for the moment, lost all recollection even of the storm itself. The sparkling of sea-water, although common, is a subject not satisfactorily explained. At the present day, naturalists maintain two theories: one goes to ascribe it to phosphorescent animalcula, of which myriads are detected, by the aid of the microscope, in every part of the ocean, though most abundantly in the vicinity of the equator. The other refers it to putrefaction: during this process, luminous sparks are copiously evolved when the water is briskly agitated. Thus the phosphorescent principle is brought forward by both parties—the former believing the light to proceed from living, the latter from dead animal matter. The luminous appearance of the ocean, in the present instance, differed materially from that which it usually presents: in addition to those small points of light which are observed to flash from sea-water when ruffled, others were readily distinguishable by the very vivid and copious rays they emitted.

"To ascertain the nature of the substance from whence so much light issued, a bucket was prepared, by means of which we were so fortunate as to dip up three. The description is as follows: they are composed of a cartilaginous-like substance, from two to three inches long, and covered with transparent eminences, each containing a drop of water: the shape slightly conical, having an opening through the base, which terminated in the opposite extremity. The internal structure proved equally simple with the external; in the place of vesicles this surface was studded with minute grains of a brownish complexion. No signs of sensibility were discoverable, and the only indication of animation was the power of ascending and descending in water; during these motions a state of contraction and dilatation were alternately perceived. Viewed in a glass of sea-water, a scene beautifully brilliant was displayed; the delicacy of cerulean, mingled with the splendour of phosphorescence, whilst numerous intermediate shades served to vary and enrich the showy prospect. This pleasing illumination was only visible whilst the substance was in motion, which might readily be induced by a gentle degree of agitation given occasionally to the water.

"To preserve one of these extraordinary dazzlers it was immersed in spirits; in a short space of time the sole relics of its late splendour consisted of an unmeaning cylinder of colourless cartilage."

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE,

BY MR. SAMUEL VEALL.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1822.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS			
Jan. 15	22	38·5	30·	Stormy
16	23	35 5	29·95	Fine
17	24	35·5	30·	Cloudy—snow A.M.
18	25	45·	30 05	Ditto
19	26	45·5	30·10	Ditto
20	27	49·5	29·93	Fine
21	28	47·	30·12	Very fine
22	29	45·	30·15	Cloudy
23	new	47·	29·88	Ditto
24	1	47·5	29·65	Ditto
25	2	44·	29·60	Rain
26	3	43·5	29·90	Fine
27	4	37·	30·20	Cloudy
28	5	49·	29·95	Fine
30	7	40·	30·18	Ditto
31	8	43·5	30·05	Ditto
Feb. 1	9	46·	29·85	Ditto
2	10	51·5	29·30	Stormy—violent storm P.M.
3	11	45·	29·26	Fine—violent storm A.M. with rain
4	12	42·	29·55	Cloudy—rain at night.
5	13	49·	28·82	Stormy
6	full	41·	29·93	Fine
7	15	47·5	29·55	Cloudy—rain at night.
8	16	40·	29·60	Fine—rain A.M.
9	17	48·	29·58	Cloudy
10	18	48·5	29·65	Ditto—rain A.M.
11	19	47·5	29·80	Fine
12	20	43·	30·10	Ditto
13	21	41·	29·92	Cloudy
14	22	46·	29·85	Ditto

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND.

Days of Month. 1822.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Weather.
Dec. 27	33	43	42	30·37	Fair
28	46	51	41	·20	Fair
29	37	47	41	·28	Fair
30	32	40	33	·39	Fair
31	32	44	38	·35	Fair
Jan. 1	42	47	42	·20	Fair
2	46	49	50	29·73	Small, with wind
3	43	46	36	·52	Fair
4	33	45	49	·66	Cloudy
5	47	51	35	·33	Stormy
6	33	46	40	30·19	Fair
7	44	50	47	29·86	Fair
8	46	47	45	·95	Fair
9	46	50	47	·86	Cloudy
10	47	53	46	·85	Fair
11	42	47	45	30·07	Fair
12	38	42	39	·26	Foggy
13	38	50	43	·15	Fair
14	43	46	41	·12	Fair
15	41	53	47	·12	Fair
16	39	48	45	·44	Fair
17	44	54	45	·42	Fair
18	46	54	46	·40	Fair
19	41	48	42	·44	Cloudy
20	45	46	43	29·91	Cloudy
21	37	46	38	30·45	Fair
22	36	46	41	·36	Fair
23	41	48	40	·32	Fair
24	40	50	49	·20	Fair
25	50	53	47	·27	Cloudy
26	46	50		·16	Cloudy

N.B. The Barometer's height is taken at one o'clock.

Observations for Correspondent who observed the

11th Jan.	8 o'Clock	M. Barom.	30 036	Ther. attached	51°	Detached 42
— —	9	— — — —	·035		50	— — 48
— —	11	— — — —	.064		50	— — 47
— —	1	— N. — —	·070		50	— — 47

XXXVI. *On the Theory of parallel Lines in Geometry.* By JAMES IVORY, *M.A. F.R.S.*

In laying down the elements of mathematical science, great difficulties occur at the outset. In arithmetic we are immediately embarrassed with the doctrine of incommensurable quantities. In geometry, the manner of treating the subject of parallel lines is a blemish which the efforts of ancient and modern mathematicians have equally failed to remove. In the same science some obscurity and even mistakes prevailed with respect to the equality of solid figures, till accuracy and precision were introduced by the publication of Legendre's Elements, one of the ablest and most original works that has appeared in modern times. The comparison of the pyramid with the prism must also have occasioned some perplexity to the first authors who wrote on geometry. In advancing further, new difficulties occur at every step; as when we would compare the lengths of curve and straight lines; or when we would determine the proportion between curve and plane surfaces.

In algebra some obscurity has arisen from what are called negative quantities. But it would be very inaccurate to assimilate the seeming paradoxes and apparent contradictions that arise from the doctrine of negative quantities in algebra to the real difficulties that are met with in geometry. The latter are unavoidable, and inherent in the subject. the former originate from inaccurate phraseology, and the crude and unphilosophical manner of treating the elements of a branch of science comparatively new, and that, in no great space of time, has been almost immeasurably extended. There can be no better argument for the truth of what is here advanced, than to observe that the absurdities attending the use of the negative sign appear only in general discussions, and when the quantities affected with it are considered abstractly. It is only on such occasions that we hear of quantities less than nothing; or that negative quantities are compared to debts, while positive quantities signify real possessions. These exceptionable modes of speaking are never introduced in the solution of particular problems. In such cases all the relations of the quantities considered are distinctly comprehended, and the algebraist readily accomplishes his purpose by means of addition, and the no less clear operation of subtracting a less from a greater quantity. It is with these clear notions that the mind, in every investigation, sets out; and the difficulty consists in reconciling them with the generalizations not only permitted but required by the genius of algebra.

Some mathematicians of this country, founding their objections chiefly on verbal inaccuracies, contend that the doctrine of negative quantities should be banished from algebra. Without stopping to inquire into the cause of the obscurities of which they complain, these purists will give no quarter to any thing that has even the appearance of infringing the clearness and evidence which is the boast of mathematical science. They rather choose to obviate the difficulties they meet with, by breaking down every proposition into its particular cases, than, by following the spirit of the algebraic analysis, to comprehend them all in one investigation. In order to obtain the same clearness in algebra for which the ancient geometry is admired, they would neglect the distinction between the two sciences, and would cramp the former by the restrictions to which the latter is necessarily subject.

In geometry every proposition, even the most general, is demonstrated with reference to a particular diagram. In the 47th of the first book of Euclid, all the reasoning is directed to the particular triangle represented in the scheme. But as no part of the demonstration depends upon any peculiar relations of the sides or angles of that triangle, it is clearly seen that the property proved will belong to any one of the same species. In the instance now mentioned there are no subordinate cases that require an alteration of the diagram. But as one geometrical figure can properly represent all those only that are exactly similar to it, it often happens that the different cases of the same proposition require several diagrams, to each of which a separate demonstration must be applied. The geometer may perceive a great similitude between the subordinate cases; insomuch that, when one is understood, all the rest are readily deduced from it; but the science he cultivates furnishes no method of bringing the observed analogy under precise and general rules. A geometrical demonstration is never deemed complete, unless all the cases be fully enumerated, and separately investigated.

The algebraist can no more translate a problem from the common into the analytical language, without conceiving a particular state of the quantities concerned, than the geometer can demonstrate without reference to a particular diagram. But when an equation has been obtained from one particular case, it necessarily comprehends under it every possible case of the same problem. In algebra there can be no variation in the state of a problem excepting as the quantities concerned are greater or less. If the quantity sought be greater than some known quantity, an addition is implied; if less, we must conceive a subtraction. But an equation obtained on the first hypothesis, applies to

to the second, by the substitution of a negative in place of a positive quantity; that is, merely by changing the signs of some of the terms; which changes are made not in an arbitrary manner, but by fixt rules derived from the mechanism of analytical language. This conclusion was not perhaps perceived in all its extent by the first algebraists, but it has been firmly established in the progress of the science, and is indeed a necessary consequence of the general rules about which all are agreed. It is in this manner that an algebraic expression, the structure of which remains essentially the same, adapts itself to all the possible cases of a problem, while in geometry the same cases are only connected by a vague similarity not reducible to precise rules. When an equation is solved, the result may either be positive, that is, a quantity to be added; or it may be negative, that is, a quantity to be subtracted; but in both cases the meaning is equally clear when we go back to the primitive hypothesis, and consider the algebraic signs as notes of reference to the different views that may be taken of the same problem.

Algebra therefore, by means of the doctrine of negative quantities, possesses a great advantage over geometry. In the former science, a problem is comprehended in one expression capable of adapting itself, by the regular changes it admits of, to every particular case; in the latter, all the subordinate cases remain detached, and must be separately considered. By the comprehensive spirit of the first science, the investigation of truth is shortened and facilitated. Nor does it necessarily follow that the generalizations of algebra must be attended with obscurity. It may be affirmed that the ideas of the algebraist are clear in many instances where, by using the language of his predecessors, he has expressed himself in terms the most exceptionable.

The mathematicians, who would reject negative quantities, would introduce into algebra the procedure, necessarily followed in geometry, of minutely subdividing every proposition into all its particular cases. By this means no doubt the same clearness would be obtained in the one science which is so commendable in the other; but at the same time algebra would be stript of its greatest and most peculiar excellence as an instrument for the investigation of truth. What the purists recommend is, in reality, to cut the knot of the difficulty, in order to avoid the trouble of unravelling it. The proper remedy seems to be, to mount up to the cause of the imperfections complained of, and by an enlarged view of the nature and scope of the science, to preserve all the generality of which it is capable, at the same time that its rules are deduced with the evidence required in mathematical reasoning.

But, to return to geometry, it may be worth while to inquire, what has been the conduct of the ancients in regard to the difficulties that present themselves in that science. They have either overcome the obstacles that obstructed their progress; or, when this was impossible, they have fairly laid down what they could not demonstrate as principles to be assented to by their disciples in the further prosecution of their researches. Of the first of these ways of proceeding we have instances in the investigations relating to incommensurable quantities, and to the proportion between the pyramid and the prism. In both these cases too the ancient geometers have succeeded by the same means, namely, by employing the indirect mode of investigation.

Archimedes furnishes an example of the other way of proceeding in the *principles* prefixed to his treatise on the Sphere and Cylinder. These principles, on which are founded the most considerable of his discoveries in pure mathematics, are really theorems which ought to be demonstrated, but which the ancient geometry affords no means of proving. Another instance of the same kind we have in Euclid's manner of treating parallel lines. That geometer has demonstrated, in the 17th of the first book, that any two angles of a triangle are together less than two right angles. The plan of his work required that the converse of the same proposition should be proved; and it is the want of this proof, which no geometer has been able to invent, that constitutes the difficulty in the theory of parallel lines. Euclid has therefore, in the 12th Axiom, laid down, as a principle to which the assent is demanded, the proposition of which no demonstration can be found. It is no doubt inaccurate to class a principle of this kind with the axioms to which it has no affinity; but this is an objection of no moment, when the intention of the author is understood.

Many attempts have been made by succeeding geometers to remove the defect found in Euclid's doctrine of parallel lines. New definitions of the straight line have been imagined; and new axioms, or rather new principles of reasoning, have been proposed; but none of these expedients have been attended with complete success. On a deliberate view of the case the preference must, I think, be given to Euclid's manner of treating the subject; because it places before the student without disguise the true nature and origin of the difficulty.

Legendre, in the first nine editions of his Geometry, has treated parallel lines in a manner that is both new and seems to be more intimately connected with the real cause of the difficulty than any other hitherto proposed. The foundation of it is to prove, inde-

independently of the theory of parallels, that the three angles of a triangle are together equal to two right angles. This is accomplished by proving indirectly that the angles of a triangle can neither be less nor greater than two right angles. And, when we reflect that the whole difficulty is occasioned by the imperfect nature of the definition of a straight line, we are led to suspect that it is necessary to employ the indirect mode of reasoning. One objection may be made to Legendre's demonstration; for we are required to admit that, through a point situated within a rectilineal angle, at least one straight line may be drawn that shall meet both the sides of the angle; a hypothesis, which, although it be very probable, is yet in some degree uncertain and precarious. Thinking to gratify the lovers of speculative geometry, I shall now add a demonstration of the same proposition, which requires no new principles, and is liable to no objection excepting the length that always attends indirect investigations.

Prop. I. Fig. 5 (Plate III.)

To construct a triangle that shall have the sum of its angles equal to the sum of the angles of a given triangle, and one of its angles equal to, or less than, half any proposed angle of the given triangle.

Let A B C be the given triangle, and A B C one of its angles: bisect the side A C, opposite to A B C, in E; join B E, and, having produced it, cut off E F equal to B E; join C F: the sum of the angles of the triangle B F C will be equal to the sum of the angles of the triangle A B C; and one of the angles F B C or B F C will be equal to, or less than, half the angle A B C.

The construction being the same as in the 16th of the first book of Euclid, it may be proved, as in that proposition, that the two triangles A E B and C E F are equal in all respects. Wherefore, the angle B A E being equal to E C F, the whole angle B C F is equal to the two angles B A E and B C E; and, the angle A B E being equal to E F C, the whole angle A B C is equal to the two angles C B E and E F C. Consequently the three angles B C F, C B E, and E F C are equal to the three angles B A C, A C B, and A B C. Again, if B C be equal to C F, the angles E B C and E F C will be equal to one another, and to the half of A B C; but, if B C and C F be unequal, the angles E B C and E F C will likewise be unequal, and one of them will be less than the half of A B C.

This proposition may be considered as a corollary to the 16th of the first book of Euclid.

Prop.

PROP. II. Fig. 5.

The three angles of a triangle cannot be greater than two right angles.

If it be possible, let the three angles of the triangle A B C be greater than two right angles, and let the excess above two right angles be equal to the angle x. Construct the triangle B C F, having the sum of its angles equal to the sum of the angles of the triangle A B C, and one angle F B C equal to, or less than, half the angle A B C; in like manner, construct another triangle F′B′C′ having the sum of its angles equal to the sum of the angles of the triangle F B C, and one angle F′B′C′ equal to, or less than, half the angle F B C; and continue the like constructions as far as necessary. Because the angle F B C is equal to, or less than, half A B C; and the angle F′B′C′, equal to, or less than, half F B C, and so on; by continuing the series of triangles far enough, we shall at length arrive at one, viz. F′B′C′, having an angle F′B′C′ less than the given angle x*. And because the three angles of every triangle in the series make the same sum, the three angles B′C′F′, B′F′C′, F′B′C′ will be together equal to the sum of two right angles and the angle x: but the angle F′B′C′ is less than the angle x; wherefore the angles B′C′F′ and B′F′C′ are greater than two right angles; which is absurd (17.1.E.) Therefore the three angles of a triangle cannot be greater than two right angles.

The following demonstration does not fall off from the accuracy and spirit of the ancient geometry, although, for the sake of brevity, it is not dressed out in the usual *costume*.

PROP. III. Fig. 6.

The three angles of any triangle are equal to two right angles.

If what is affirmed be not true, let the three angles of the triangle A C B be less than two right angles, and let the defect from two right angles be equal to the angle x. Let P stand for a right angle, and find a multiple of the angle x, viz. $m \times x$, such that $4P - m \times x$, or the excess of four right angles above the multiple angle, shall be less than the sum of the two angles A C B and A B C of the proposed triangle. Produce the side C B, and cut off B E, E F, F G, &c. each equal to B C, so that the whole C G shall contain C B m times; and construct the triangles B H E, E K F, F L G, &c., having their sides equal to the sides of the triangle A C B, and consequently, their angles equal to the angles of the same triangle. In C A produced, take any point M, and draw H M, K M, L M, &c.; A H, H K, K L, &c.

All the angles of all the triangles into which the quadrilateral

* For by continually bisecting any proposed magnitude, a magnitude will at length be found less than any given magnitude.

figure

figure C G L M is divided, constitute the four angles of that figure, together with the angles round each of points H, K, &c., and the angles, directed into the interior of the figure, at the points A, B, E, F, &c. But all the angles round the points H, K, &c., of which points the number is $m-2$, are equal to $(m-2) \times 4P$, or to $4mP-8P$; and all the angles at the points A, B, E, F, &c., are equal to $m \times 2P$. Wherefore the sum of all the angles of all the triangles into which the quadrilateral C G L M is divided, is equal to the four angles of that figure together with $4mP-8P+2mP=6mP-8P$.

Again: the three angles of the triangle A B C are, by the hypothesis, equal to $2P-x$; and, as the number of the triangles C A B, B H E, E K F, F L G is equal to m, the sum of all the angles of all these triangles, will be equal to $2mP-m \times x$. Upon each of the lines A H, H K, K L, there stand two triangles, one above and one below; and, as the three angles of a triangle cannot exceed two right angles, it follows that all the angles of those triangles, the number of which is equal to $2m-2$, cannot exceed $4mP-4P$. Wherefore the sum of all the angles of all the triangles into which the quadrilateral C G M L is divided, cannot exceed $4mP-4P+2mP-m \times x=6mP-8P+4P-m \times x$.

It follows, from what has now been proved, that the four angles of the quadrilateral C G L M, together with $6mP-8P$, cannot exceed $6mP-8P+4P-m \times x$. Wherefore, by taking the same thing, viz. $6mP-8P$, from the two unequal things, the four angles of the quadrilateral CGLM cannot exceed $4P-m \times x$. But $4P-m \times x$ is less than the sum of the two angles A C B and A B C, or than the sum of the two angles A C B and L G F: wherefore, *a fortiori*, the four angles of the quadrilateral cannot exceed the sum of the two angles A C B and L G F; that is, a whole cannot exceed a part of it; which is absurd. Therefore the three angles of the triangle A B C cannot be less than two right angles.

And because the three angles of a triangle can neither be greater nor less than two right angles, they are equal to two right angles.

By the help of the proposition just proved, the defect in Euclid's Theory of parallel Lines may be removed, as the reader will see by consulting the notes to Professor Playfair's Elements of Geometry.

Legendre has demonstrated the same proposition in a different manner, by means of algebraic functions. The like mode of reasoning has also been applied to elementary propositions in other branches of science; particularly to the composition of forces in mechanics. The evidence of such demonstrations may, on another occasion, become the subject of inquiry.

March 6, 1822. J. Ivory.

XXXVII. *Further Observations on Mr.* LEESON's *Safety Blowpipe Appendages.*

To Dr. Tilloch.

Nottingham, Jan. 18, 1822.

SIR, — THE results of several experiments, made by me since I last addressed you, have induced me to make some alterations in my Safety Appendages, described in the 284th Number of your Magazine; and my anxiety for the improvement of the blowpipe, added to a hope of exciting the attention of persons more qualified than myself to add to the resources of science, will I trust be a sufficient apology for my again troubling you.

Instead of allowing the gas to enter through the sides of the safety cistern, as represented at H in the Plate accompanying my former paper, in Number for December, I cause it to enter through a hole drilled in the bottom of the cistern as represented at A, Plate III. fig. 4. The reason of my alteration is, that owing to the great cohesive attraction of mercury, if that be used to fill the safety cistern, the gas passes up the sides of the cistern instead of bubbling through the mercury; and when a continuous stream of gas is thus produced, an explosion will infallibly ensue if the gas within the safety cistern be ignited. When the gas enters through the bottom of the safety cistern, the apparatus is perfectly safe.—Fig. 4 is of the real size.

I have found from experiment that the cane I mentioned is not safe, as the gases readily ignited through a piece one inch and a quarter in length. Wire-gauze is therefore preferable, and the small ledge in the lid of the safety cistern will serve to support the pieces, which should be cut by a punch of a proper size, and introduced into the cylinder B before screwing the jet pipe in its place.

Although I have purposely ignited the gas within the safety-cistern, I find the apparatus as it is now described perfectly safe; nor does any injury result to the valve from the trifling expansion which then takes place.

I have used the appendages without either cane or wire-gauze, and with a jet about an inch in length, the aperture of which tapered from 1-10th to 1-30th of an inch in diameter with perfect safety.

It is always better to examine the valve, after the gas within the safety-cistern is ignited, in order to remove any mercury that may have accidentally lodged therein.

The valve I now use has no grooves on the top of the plug; a small plate being screwed on the bottom of the spindle to prevent the plug from rising too high.

I have frequently exploded about two quarts of the mixed gases

gases in tin plate vessels, and have *invariably* found that the vessels were torn open without the least danger to those present, as stated in my last communication.

In all the experiments above alluded to, I have used a mixture of nine parts by measure of hydrogen, and four parts of oxygen (prepared from hyper-oxymuriate of potash).

In justice to myself, and with reference to Mr. Murray's letter to you accompanying my paper, allow me to observe, that though Mr. Murray recommended the mercury in preference to oil or water, *both* the safety-cistern, and valve therein described were entirely my own invention; and neither suggested by nor modified from any plan of that gentleman's.

I remain, sir,
Yours most obediently,
H. B. Leeson.

XXXVIII. *Description of a new Apparatus proposed for restoring the Action of the Lungs in Cases of suspended Respiration. By* J. Moore, *Jun., Esq.*

To Dr. Tilloch.

Sir, — Having observed, in your Number for October last, a drawing and description of an apparatus by Mr. J. Murray, wherewith the action of the lungs might be restored, and having also a plan to effect the same desirable object, which I consider more complete that than gentleman's, as it does not require turning a cock to admit fresh air, but every time the pistons are raised and depressed not only gives a fresh supply to the lungs, but also withdraws the air which has been injected; I presume you will have the goodness to insert the following in your Magazine.

Let fig. 1 (Pl. III.) represent a common air syringe with a solid piston, but thus differently formed, that at the bottom of the cylinder there are two tubes, as A and B, either flexible or otherwise, each tube having a valve, that of A having its valve at C to open outward; and B having its valve at D to open inward.

Now suppose there be two of those syringes placed side by side as at fig. 2, the tubes together; thus, A in the one syringe must be placed by the side of B of the other, placing the tubes together, whose valves open contrary to each other.

In order to try the efficacy of this plan, have a common bladder, and into the neck of it pass either of two tubes which are connected, and let the piston handles be fastened together; then, as you raise and depress them, the bladder will expand and contract: you will perceive that by the upstroke of the pistons, the

air passes from the atmosphere into the one syringe, whilst the air contained in the bladder is passing into the other syringe: now, as the pistons are depressed, the airs which the syringes contain are thus disposed of: that syringe which drew air from the atmosphere injects it into the bladder, whilst the other syringe which drew air from the bladder, injects it into the atmosphere, and thus an artificial respiration is obtained, and a continual supply of fresh air may be passed into the lungs; which might be either the common atmospheric air, oxygen, or any mixture, provided the proper tube be attached to the vessel which contains the gas.

The various uses to which these syringes are applicable I will not minutely detail, but only mention briefly a few. First, exhaustion by either syringe when separate. Second, condensation. Third, exhaustion and condensation at the same time. Fourth, when the syringes are combined as above described, a change of the atmosphere which occupies the vessel in that to which they are attached. Fifth, if the two syringes are put together, so that the A tube of each syringe shall be inserted into separate vessels containing liquor or gases, whilst the other tubes B B are inserted into a third vessel, as the pistons are worked the liquor or gases will be drawn from the vessel containing the tubes A A, and be mixed in the vessel containing the tubes B B, which may be applied as a gas-blowpipe.

When the syringes are for the purpose of restoring suspended animation, it might be well to inclose one of them with a case to contain hot water, similar to the description given by Mr. J. Murray; but I should also propose that the tubes A and B, which are to be placed into the mouth, have valves in them; thus the tube A might have a valve near C to open into the atmosphere, which may be loaded or have a spring to prevent its opening when the lungs are not strained: but should a pressure be given which would be injurious to them, the valve would then open, and thus prevent it. The tube B, which also enters the mouth, may have a valve near D to open inwardly, and be similarly loaded or retained by a spring, to prevent the rarefaction within the lungs from being so complete as to endanger a rupture of any of the small vessels.

The same sort of valves may be applied to the breathing the nitrous oxide, so as to ensure its not passing to the lungs a second time, which I consider to be injurious to the individual performing the experiment.

Let E (fig. 3) represent the lower part of a syringe with its tubes either flexible or otherwise, and having valves as before described. Let F be a mouth-piece. Suppose a bladder containing the gas be attached to the tube B, and an individual in the

the act of inhaling the gas, it would pass from the bladder into his lungs, and when he exhaled, the breath would pass through the other tube that has the valve to open outward into the air.

I remain respectfully, &c.

Pratten's Row, Lawrence Hill, Bristol, Jan. 10, 1822. JOHN MOORE, Jun.

XXXIX. *Process for procuring pure Platinum, Palladium, Rhodium, Iridium, and Osmium, from the Ores of Platinum. By* M. BARUEL, *Chemical Operator in the School of Medicine at Paris* *.

1. Two sorts of platinum ore occur in commerce, one of which is white and brilliant, the other is blackish coloured. The latter contains much more iron † than the preceding; both ores exist always in the form of small spangles, which vary in size; platinum ore is one of the most compound known: besides the five metals above noted, several others are found in it, especially two kinds of ferruginous sand, one of them attractible by the magnet, the other not, and which is a combination of the oxides of titanium and iron: there is, besides chromate of iron, some copper, particles of gold alloyed with silver, with copper, and mercury. It contains, moreover, some sulphuret of lead and copper. We may hence judge of the singular complexity of this mineral, and be ready to acknowledge that its exact analysis, in regard to the proportion of its constituents, is nearly impossible. In order to separate the platinum, palladium, rhodium, iridium, and osmium, from each other, and the rest of the bodies, the following method is the one which long experience has proved most successful.

2. The ore is triturated in a cast-iron mortar for a considerable time, during which a stream of water is constantly passed over it, to wash away the ferriferous sand, the titanite, and chromate of iron, reduced to an impalpable powder. When the ore is very brilliant, it is left to settle for an instant; the water is decanted off, and it is then exposed in a crucible to a red heat during a quarter of an hour. The whole mercury is thus volatilized, when we can readily distinguish the spangles of alloy of gold and copper by their colours.

3. The calcined ore being introduced into a tubulated retort, we pour over it half its weight of nitro-muriatic acid (aqua regia)

* From Mr. Brande's Journal of Science. This valuable memoir derives peculiar interest from the large importation of the above ore daily expected from South America, in consequence of the negotiation between M. Zea and some London merchants.

† Rather, the fine black powder, or ore of iridium and osmium, noticed in paragraph 8.—Tr.

composed

composed of one part of nitric acid at 25° Baumé (1·210 sp. gr.) and three parts of muriatic acid, at 18° (1·14), and heat the mixture for half an hour. Such acid dissolves all the gold, all the lead, the greater part of the copper, and a very small quantity of platinum, palladium, and iron, while the silver is converted into a chloride, which remains mingled with the ore not attacked. After decanting the acid liquor, the ore is thrown on a filter, and washed with a sufficient quantity of water. The filter-funnel being transferred to another vessel, the filter is to be washed with a very weak water of ammonia. By this means we dissolve all the chloride of silver, which is recovered by saturating the filtered liquor with muriatic acid.

4. The solution which contains the gold, lead, copper, and iron, with a small quantity of palladium and platinum, being added to the water which has served for the washings, the whole is now evaporated to the consistence of syrup, which is diluted with thrice its volume of water, and treated with sulphuric acid, drop by drop, to precipitate the lead in the state of sulphate, to be afterwards separated by the filter.

5. Into the filtered liquor a solution of proto-sulphate of iron must be poured, which throws down the gold and palladium in the metallic state. We decant the liquor, wash and dry the precipitated metals. The platinum remains in the liquor with the iron and copper. We concentrate this liquor by evaporation, then pour into it a sufficient quantity of a saturated solution of muriate of ammonia, which throws down the platinum in the state of ammonio-muriate. This must be washed on a filter and dried.

6. The gold may be very easily separated from the palladium by melting these metals with four times their weight of silver, and acting on the alloy with concentrated nitric acid, which dissolves the palladium and silver, but leaves the gold in the form of a brown powder, which may be fused into a button in a crucible. Into the nitric solution of silver and palladium we pour muriatic acid, which throws down all the silver in the state of chloride. The liquid freed by the filter from the chloride contains only palladium. We add to it a few drops of solution of sal-ammoniac, then saturate the redundant acid by ammonia; the whole palladium is thus precipitated in the state of an ammonia proto-submuriate of palladium, which exhibits small needles, of a delicate rose colour. This salt is to be washed on the filter, and dried.

7. The ore of platinum which has been successively treated with weak nitro-muriatic acid, and then with ammoniacal water, to carry off the chloride of silver, is to be strongly desiccated. Having replaced it in the retort, we pour over it a weight equal to its own of nitro-muriatic acid, made in the same proportion as the

the above, but with this difference, that the acids ought to be as concentrated as possible. I employ for this purpose nitric acid, at 40° (1.387 sp. gr.) and muriatic acid, at 23½° (1.195). The retort is placed on a sand-bath, with a tubulated receiver adapted to its neck, and it is heated moderately. A brisk effervescence soon arises, owing to the disengagement of much nitrous vapour, and a little chlorine. The action of the heat must be so modified as to produce the most beneficial effect on the solution, without volatilizing the acid. Finally, when the effervescence ceases, the fire is to be augmented till the liquid boils, and till no more orange nitrous fumes are disengaged.

When the action of the acid is quite exhausted, we decant the liquid into a matrass, and pour on the portion of the ore not attacked the same nitro-muriatic acid, equal in quantity to the first.

The mixture is to be heated anew, observing the same precautions as for the preceding solution. Finally, we treat the ore five times in succession with the compound acid. By this process six parts of this acid are sufficient to dissolve the whole platinum, palladium, and rhodium contained in the ore.

8. After the last digestion, which yields only a slightly reddish-coloured solution, there remains a residuum, under the form of a brilliant blackish powder, which consists of an alloy of iridium and osmium. One part of this is a fine powder (see Note to paragraph 1), and the other forms brilliant spangles. We shall return in the sequel to the residuum; let us employ ourselves at present on the solution.

9. We have said that all the platinum, rhodium, and palladium were dissolved; but the acid also dissolves a little iridium and osmium, as well as the iron alloyed with the platinum grains. During the action of the acid on the ore, at the same time that the nitrous gas and chlorine are evolved, there is volatilized a little water and muriatic acid, which carry over with them a notable quantity of oxide of osmium, which is condensed in the receiver.

10. All the successive solutions of the ore of platinum are united and introduced into a retort of proper capacity, to which the receiver containing the former condensed vapours is attached. The retort is now heated on a sand-bath, till its contents acquire the consistence of syrup. By this means we drive off all the excess of the acid, which carries along with it into the receiver the whole oxide of osmium which that solution contained.

11. The product of the last distillation being saturated with lime, we distil over to one-half the volume. The product of this new distillation has an extremely penetrating odour, on account of

of the large proportion of oxide of osmium which it contains. It must be preserved in glass bottles, furnished with well-ground stoppers.

12. The concentrated solution of platinum is to be diluted with from five to six times its weight of water, then filtered.

13. The black powder which was not acted on by the nitro-muriatic acid, is also to be washed with water, dried, and kept in a phial; we shall distinguish it by the name of the *black powder*.

14. Into the filtered solution we pour a saturated solution of muriate of ammonia, till this ceases to occasion any precipitate. In this operation there are formed ammonio-muriates of platinum, iridium, rhodium, and palladium. These two last salts being very soluble, remain in the liquid with the iron; but the ammonio-muriates of platinum and iridium being very sparingly soluble form the precipitate, which has a tawny or reddish-yellow colour of more or less depth, according as the proportion of the salt of iridium is more or less considerable. When the further addition of the muriate of ammonia produces no more precipitate, the whole is to be thrown on a filter of cotton, and washed with water of as great coldness as possible, which is conveniently procured by putting a bit of ice into the water intended for the washings. When the precipitate is sufficiently washed, which is recognised by the water that passes having merely a faint yellowish hue, it is to be dried. This precipitate, as we have remarked, is an ammonio-muriate of platinum, the pure yellow of which is altered by its mixture with the ammonio-muriate of iridium, which is red.

15. This impure ammoniacal salt of platinum is calcined in a crucible, observing to heat the crucible at first in its upper part, in order to avoid the volatilization of a portion of the salt, without its being decomposed. The heat is to be pushed to redness, at which temperature it must be kept up for an hour. By this means the salts are decomposed, and there remains in the crucible only the platinum and iridium. To separate these two metals we put them into a retort, and dissolve them anew in the nitro-muriatic acid; but in this case the nitric acid must be only at 28° (1·24), and the muriatic acid at 19° (1·15). Two and a half parts of this acid suffice to dissolve one of platinum thus reduced, without affecting the iridium. This metal remains at the bottom of the liquor (which is of a fine orange-yellow colour) under the form of a grey powder. On filtering, pure iridium remains above, which is to be washed and dried.

16. The solution of platinum must be precipitated once more by muriate of ammonia; and the fine yellow ammonio-muriate of

of platinum thus obtained, is to be reduced by strong calcination in a crucible, observing the precautions already indicated. The pure platinum remains in the crucible, under the form of a greyish-coloured spongy mass, which acquires metallic lustre by friction against any hard body.

17. As platinum can be fused only in small masses at a time, and at a flame supplied with oxygen gas, or the compound flame of oxygen and hydrogen, it cannot be melted on the large scale like most others. However, chemists have succeeded in forming this metal into ingots of a very considerable weight, by uniting the particles with strong pressure at a very high temperature. For this purpose, a certain quantity of platinum, resulting from the calcination of the triple ammoniacal salt, is compressed in a crucible; then more is successively introduced, even to the amount of 20 or 30 pounds. The crucible is then covered, and heated to whiteness. The platinum is now transferred as speedily as possible into a square steel matrix (a strong hoop of steel, jointed, would answer equally well) and capable of opening into two pieces by means of hinges. On the top of the ignited mass, a steel mandril, adapted to the cavity of the matrix, is to be applied, which is to be rapidly driven home, by three or four blows of a strong coining screw-press. By this powerful pressure, which the spongy platinum experiences at a white heat, it diminishes greatly in bulk, and its particles already acquire a pretty strong cohesion. The matrix or collar is opened, the mass of platinum is removed to be heated anew in a crucible to a red-white heat, at a fire acted on by two good bellows. It is again introduced with the utmost celerity into the matrix, where it receives five or six blows of the fly-press. In the second operation, all the particles of the platinum are sufficiently approximated to form a homogeneous mass, which may be thenceforth heated, without inconvenience, among naked charcoal, giving it the greatest possible heat, and condensing, with two blows of the press, each face of the ingot. In thus transferring the mass of platinum successively from the forge to the press about thirty times, we obtain an ingot perfectly sound, possessed of great malleability and ductility. Platinum thus made into ingots, is delivered to the workmen, who fashion it like gold and silver; that is to say, all the pieces are stretched at first under the rolling-press, and then fashioned by the hammer, taking care to anneal it from time to time. Thus are prepared, in France, the great masses of platinum, with which are fabricated the large alembics destined for the concentration of sulphuric acids.

18. The mother-water, from which have been precipitated the ammonio-muriates of platinum and iridium by pouring muriate

of

of ammonia into the solution of crude platinum, has a reddish-brown colour, and contains all the ammonio-muriates of palladium and rhodium, as well as a certain quantity of the ammonio-muriates of platinum and iridium; because, as we have observed, these salts are not completely insoluble. It contains, moreover, all the iron which was alloyed with the platinum, and sometimes a little copper, which has escaped the action of the first portion of nitro-muriatic acid which was poured on the ore to dissolve the gold. This mother-liquor is put into matrasses, and plates of iron are plunged into it. The iron precipitates all the metals (except the oxide of iron) under the form of a black powder. When the whole metallic matter is thrown down, which is known by the liquor assuming a green colour, the plates of iron are removed, after detaching from their surfaces the adhering powder. The liquor is decanted off, and thrown away. The black precipitate must be washed several times, till the water employed passes off tasteless. The powder is then treated with weak nitric acid, which dissolves the greatest part of the iron, which, by the effect of the precipitation, had been alloyed with these metals*, and which takes up also whatever copper may remain. The residuum is washed anew, and treated with nitro-muriatic acid, which dissolves all the platinum, palladium, rhodium, and remains of the iron; but does not affect the iridium, which remains pure at the bottom of the solution in the form of a black powder, or metallic spangles. The iridium, being separated by the filter, is then washed, dried, and united to that formerly obtained (15).

19. The liquors are now to be united, and evaporated to the consistence of syrup, to drive off the greater part of the acid excess; then this is to be diluted with four or five times its weight of water, as cold as possible. Into this a solution of muriate of ammonia is to be poured, till it ceases to occasion a precipitate. What falls is an ammonio-muriate of platinum, which must be separated by filtration. The solution is then concentrated, and allowed to cool several times in succession, to separate all the ammoniacal salt of platinum which it may contain. When the liquid is completely deprived of platinum, or when it yields no longer the yellow precipitate, we dilute it with five or six parts of cold water; and it ought to have a sensible excess of acid. This, if wanting, may be supplied by adding a little of the muriatic. We then pour into it water of ammonia, drop by drop, but not so much as entirely to saturate the acid-excess. Imme-

* Or, *during the precipitation had fallen down in alloy with these metals.* The original words are, "fer, qui par l'effet de la précipitation s'étoit allié avec ces métaux."

diately

diately there is formed, in the liquid, a precipitate in the shape of small needles, delicate and shining, possessing a beautiful pale rose-colour. This crystalline precipitate is an ammonio-sub-protomuriate of palladium. Since this salt is insoluble, there can remain none of it in the liquid. It may be separated by the filter, and washed with very cold water. By heating this salt to redness in a crucible, the palladium remains pure. It may be afterwards melted in a cavity of ignited charcoal, on which a stream of oxygen gas is made to play.

20. The liquid freed from the salt of palladium, possesses a fine currant-red colour, derived from the ammonio-muriate of rhodium, which it holds in solution, and which is very soluble. It contains, moreover, a little muriate of iron, and occasionally a little muriate of copper, when this metal has not been entirely dissolved by the first portion of nitro-muriatic acid, which was made to act on the ore, as has been stated above. There are two modes of treating this salt, to obtain pure rhodium. The first consists in evaporating this liquid, at a gentle heat, to dryness; and boiling the residuum several times along with absolute alcohol. The spirit dissolves all the muriate of iron and copper, with the excess of sal-ammoniac, and does not affect the ammonio-muriate of rhodium, which remains in the form of a saline powder of a fine carmine-red colour. By calcining this salt to redness in a crucible, we decompose it, and the rhodium remains pure and perfectly metallic. The second means of obtaining the rhodium from the above liquid, consists in plunging into it plates of iron. The rhodium and the copper are precipitated, carrying down with them a little iron. When everything is fallen down, the liquor is decanted, the precipitate is washed, and boiled with an excess of strong muriatic acid, which dissolves all the iron. The liquid is now poured off, the residuum is washed with a sufficient quantity of water, and is next boiled several times with concentrated nitric acid, which dissolves all the copper. The rhodium being completely insoluble in each of these acids separately, remains under the form of shining pellicles, which must be washed and dried. Rhodium being the most infusible of metals, cannot be melted but in small pieces, by the aid of a flame fed with oxygen gas, or by the compound flame of hydrogen and oxygen.

21. Let us return to the black powder separated from the platinum ore, by treating it with nitro-muriatic acid. We have said that this black powder was an alloy of osmium and iridium. It is scarcely affected by any nitro-muriatic acid. It requires, indeed, an enormous quantity of this acid to dissolve a minute particle of it. The only means of attacking this alloy, is to cal-

cine it with nitrate of potash. With this view, we triturate the black powder with twice its weight of a mixture of three parts of nitre and one of caustic potash, and introduce the whole into a silver crucible, which is to be kept at a cherry-red heat for half an hour. In consequence of the affinity of the potash for the oxides of osmium and iridium, the nitric acid of the nitre is decomposed, and oxidizes these metals. The crucible is to be withdrawn from the fire, allowed to cool, and cold water is then poured on the materials. This dissolves the potash, the whole oxide of osmium, and a little of the oxide of iridium. The whole being thrown on a filter, the oxide of iridium remains above, which is to be washed and dried.

22. The filtered liquor which contains the combination of potash and oxide of osmium, as well as a little oxide of iridium, is put into a flask, and saturated with nitric acid. The liquid is then put into a retort, to which is fitted a tubulated globe, surrounded with moistened cloths. On distilling, the water which rises in vapour carries with it all the oxide of osmium. When the liquid is two-thirds drawn over, the whole osmium is usually volatilized. The liquid remaining in the retort contains the nitrate of potash, and a trace of iridium. The aqueous solution of osmium is as colourless and limpid as distilled water. It has a strong and peculiar odour, extremely irritating to the nostrils, and which it is dangerous to inhale for any length of time. In order to obtain the osmium from this solution, it is put into a matrass, and we add a little muriatic acid to acidulate it slightly, and then insert a plate of pure zinc. The oxide of osmium is decomposed by the zinc, which is dissolved in the muriatic acid, and the osmium is precipitated to the bottom of the liquor in the form of a blackish-blue powder. When the oxide of osmium is completely decomposed, which may be recognised by the liquid losing its odour, we decant the fluid, pour the powder of osmium on a filter, wash it copiously with water, dry it, and put it immediately up in a well-stopped phial.

23. The oxide of iridium, proceeding from the calcination of the black powder with nitre and potash, which remained on the filter, is by no means pure. It is a mixture of oxide of iridium, of a certain quantity of the black powder, or alloy of osmium and iridium, which has not been affected by the nitre, and a little oxide of silver, derived from the crucible. This mixture is to be treated with nitro-muriatic acid, which dissolves only the oxide of iridium, converts the oxide of silver into a chloride, and does not act on the alloy. We next filter and wash. The unattacked alloy, and the chloride of silver, remain on the filter. This residuum is to be washed with water containing a little ammonia,

which

which dissolves the chloride of silver, while the alloy of osmium and iridium remains pure. This may be again calcined with the mixture of nitre and potash, to decompose it completely.

24. Into the solution of iridium, which is of a very deep reddish-brown colour, muriate of ammonia is to be poured, and the liquid is to be evaporated to dryness, at a gentle heat. The residuum is to be then treated with alcohol very highly rectified, which takes up the excess of sal ammoniac, and occasionally a little muriate of iron; because the alloy sometimes contains a little of this metal. When the alcohol is no longer coloured, the ammonio-muriate of iridium remains pure. It is necessary merely to calcine it strongly in a crucible to have pure iridium. This metal, being more infusible than rhodium, can be melted only in very small quantities by the oxygen on charcoal, or hydrogen blow-pipe.

XL. *Observations on Mr.* Newton's *Articles on Algebra, published in our January and February Numbers. By* A Correspondent.

To Dr. Tilloch.

Sir, — Having read in the Philosophical Magazine for January and February last, two letters on algebraic Addition and Subtraction, I beg leave to offer a few remarks on the subject.

The writer of the letters here referred to observes, that "the operations of addition should be restricted to quantities, whether like or unlike, which have like signs; and that that part of addition which consists in collecting quantities, whether like or unlike, which have unlike signs, should be classed under the rule for the subtraction of simple quantities." Thus, to find the sum $(n-m)$ of n and $-m$, is, according to the observation above quoted, called an example under the rule of subtraction. In order to judge of the propriety or impropriety of classing such an example under such a rule, we must consider whether the nature of the *proposition* contained in the example corresponds with the *definition* of the rule under which such example is placed; for it is not the manner of working the example, but the thing therein proposed to be done, that must point out the rule to which it (the example) belongs. Now as subtraction consists not in finding the *sums* but the *differences* of quantities, it is speaking quite contrary to the definition of the term (subtraction) to call that an example under subtraction, in which (example) it proposed merely to collect quantities together, be the nature of those quantities what it may. Hence the above

example

example does not fall under the rule of *subtraction;* and hence the operations of algebraic *addition* cannot be "restricted" to quantities of like signs; neither can the operations of algebraic *subtraction* be so "restricted:" hence a *mixture* of operations must inevitably take place under each rule: and hence it is fairly inferred, that the terms addition and subtraction are not sufficiently comprehensive in meaning to denote that *mixture* of operations coming under each of these rules.

March 1822. I am, sir, &c.

W. X. Y.

P.S. I have confined my remarks to the author's first letter, because his second appears to carry its answer along with it.

XLI. *Remarks on the Apparatus for restoring the Action of the Lungs. By* John Murray, *F.L.S. M.W.S., &c. &c.*

To Dr. Tilloch.

Feb. 15, 1822.

Sir, — You were good enough to admit into your pages some remarks of mine on the important subject of suspended animation, accompanied with a sketch of my invention: a finished form of that apparatus, it has already been stated, was presented by me to the Royal Humane Society. That invention embraced all the desiderata which, as far as occurred to my mind, could be accomplished by mechanical means, with provision for the occasional introduction of chemical agency. I founded my deductions on Dr. Carson's very ingenious and beautiful description of the machinery of the lungs, and Messrs. Allen and Pepys's accurate researches on respiration; and I conjoined my own experiments on suspended animation with these.

That I had "a single eye" to the cause I meant to serve, is an inference warranted by my own feelings, and one which will be fully sanctioned by those who known me best. The simple approbation of the Royal Humane Society was the only return in expectancy. I would indignantly spurn every other consideration in what I conceive to be an imperative duty.

The following is introduced, I am free to confess, with a view of bringing the question of suspended animation fairly before the public, and of soliciting objections (if there do exist any well-grounded counter opinions) to the apparatus which I have recommended for restoring the action of the lungs. Having thrown down the gauntlet, I shall endeavour to answer these counter conclusions (provided always the antagonist be *non-anonymous*) with what skill I may: if worsted in the conflict, I trust I shall not be found to persist in error.

Con-

Considerable service to the cause *must* accrue from agitating this important topic, which has too long slept in inglorious repose. It may lead to an improvement or still greater simplification of the mechanism I have advocated. The "bellows" but ill indeed fulfills its purpose, and on a future occasion I shall adduce such considerations and reasons as *may* prove it so, and thus have to refer to some experiments of my own, which may be at once interesting and useful.

The following is a copy of the first official reply I received from the Secretary of the Royal Humane Society, dated 10th of December last:

"Your polite communication and apparatus were laid before the Monthly Committee, who have instructed me to lay it before the Medical Committee for their opinion, and in the mean time to convey their best thanks to you for the interest you have taken in the important cause of suspended animation," &c.

This is so far well: the other communication from the Royal Humane Society, dated 2d instant, is more equivocal, and less flattering to hopes founded on such disinterested motives as are mine:

"I have the honour to inform you, by the instructions of the Medical Committee, that I laid your apparatus before them for their opinion; and that, having duly considered the same—It was resolved, 'That the form of the bellows used by the Society was preferable to Mr. Murray's apparatus, in the opinion of the Committee, as an instrument to be generally recommended for inflation.'—There are two things very important in all apparatus of the kind, to be *generally* recommended by the Society, viz. simplicity and cheapness. The first, that there may be as little obstacle as possible, in a moment which is generally that of confusion and trepidation; and the latter, that the funds of the Society may be adequate to the greatest possible extension of its means of doing good." The Secretary is pleased thus to continue: "I think your apparatus very ingenious, and very well calculated to make experiments on animals relative to the subject of resuscitation; it admits of variety, and may lead to some valuable facts: and, in the hands of a person accustomed to its use, it may be applied readily to cases of suspended animation in the human subject," &c.

Now, if I am to understand that the only objections to its *general* adoption are the *cost* and *complexity* of the mechanism, I engage to prove that in *both* these particulars the advantage rests with my apparatus. As to price, if I am not misinformed, that now recommended would be a *fraction* only of the cost of the other "used by the Society;" and as to the question of its com-

complexity, it is simplicity itself; the veriest hind could use it. "It admits of variety," indeed, and that of an important description when the combining circumstances, which aid or defeat the return of the "answering spirits back from death," are considered in all their varied phænomena.

Should I receive no comment or elucidation from any friend embarked along with myself in the same grand cause of humanity, through the medium of your pages, I shall at my early leisure point out the essential points in which I conceive the "bellows" deficient, and the numerous advantages which I believe attendant on the employment of the apparatus I have invented and presume to recommend. My inferences shall be deductions drawn from experiments instituted by myself: and also well-authenticated proofs, from other sad and unsuccessful cases, of the inutility of the "bellows." My ardour in the cause is too powerful to be chilled by hypothetical opinion, and too vivid to be quenched *without a cause*.

I have the honour to be, sir,

Your very faithful and obliged servant,

J. MURRAY.

XLII. *On the Solar Eclipse which will happen on the 28th and 29th of November* 1826; *being the principal Results of a Calculation for Greenwich. By Mr.* GEORGE INNES *of Aberdeen.*

To Dr. Tilloch.

SIR, — I SEND you for insertion in your Magazine, the results of a calculation for Greenwich, of the solar eclipse which will happen on the 28th and 29th of November 1826. The elements have been found from the Solar Tables of M. Delambre, and the Lunar Tables of M. Burckhardt.

Although the moon's apparent semidiameter exceeds that of the sun, yet, by reason of the moon's great north latitude, the eclipse will not be total at any part of the globe, as the central path of the penumbra will pass beyond the north pole. For the same reason, at those places where the eclipse will be visible, the parallaxes in latitude will not be very different; and therefore also the digits eclipsed will be nearly the same to all places in Great Britain: but as the moon will be at a considerable distance from the nonagesimal, the times will be affected by the parallaxes in longitude, which vary with the situation of the place.

The elements are as follow:

Ecliptic

	D.	H.	′	″
Ecliptic conjunction, mean time at Greenwich, November	28	23	25	28·51
Equation of *mean* to *apparent time* at conjunction			11	31·26
Hence the *apparent time* is	28	23	36	59·77
Longitude of the sun and moon from the true equinox		246°	46	21·83
Sun's right ascension		244	55	41·04
—— declination, south		21	27	34·47
—— horary motion in longitude			2	32·09
—————— right ascension ..			2	41·06
—————— declination ..			+	25·65
—— semidiameter			16	15·15
—— horizontal parallax				8·93
—— latitude				0·00
Horary decrease of the equation of time ..				0·895
Obliquity of the ecliptic		23	27	36·86
Moon's latitude at conjunction, north increasing, ...		1	12	27·81
—— equatorial horizontal parallax ..		1	1	23·66
—— horizontal semidiameter			16	43·80
—— horary motion in longitude at conjunction			38	5·80
—— horary motion in longitude for the hour preceding			38	5·865
—— horary motion in longitude for the hour following			38	5·735
—— horary motion in latitude at conjunction			+3	25·833
—— horary motion in latitude for the hour preceding			+3	26·054
—— horary motion in latitude for the hour following			+3	25·613
Angle of the relative orbit with the ecliptic		5	30	36·4
Moon's horary motion from the sun in the relative orbit			35	43·62

Table of the principal Results obtained in calculating for Greenwich.

	For the appar. conjunction.		For the beginning.		For the end.	
	h. ′ ″	h. ′ ″	h. ′ ″	h. ′ ″	h. ′ ″	h ′ ″
Instants assumed	23 5 0·25	23 6 0·23	21 58 1·24	21 59 1·23	0 10 59·26	0 11 59·23
Sun's longitude	246° 45 0·72	246° 45 3·25	246° 42 10·88	246° 42 13·42	246° 47 48·01	246° 47 50·55
Moon's longitude	246 26 2·63	246 26 40·73	245 43 29·53	245 44 7·64	247 7 57·08	247 8 35·17
——— latitude	1 10 37·59	1 10 41 03	1 6 45·58	1 6 49·04	1 14 24·32	1 14 27·75
Sun's right ascension	244 54 15·14	244 54 17 82	244 51 15·29	244 51 17·98	244 57 12·31	244 57 14·99
Right ascension of the merid.	231 9 18·89	231 24 21 27	214 21 33·89	214 36 36·43	247 42 1·21	247 57 3·44
Altitude of the nonagesimal ..	24 31 28·0	24 25 29·0	31 5 7·0	30 59 7·7	18 52 36·3	18 43 13·8
Longitude of the nonagesimal..	199 7 43·6	199 25 52·2	181 26 37·8	181 40 43·7	222 50 7·6	223 15 53·4
Parallax in longitude ......+	18 44·74	18 35·42	28 33·16	28 25·00	8 10·93	8 1·18
——— in latitude	55 32·22	55 34·70	52 17·51	52 20·78	57 45·15	57 46·61
Appar. diff. long. ⊙ and ☽ ..	13·35	12·90	30 8·19	29 40·78	29 20·00	28 45·80
Moon's apparent latitude N...	15 5 37	15 6·33	14 28·07	14 28·26	16 39·17	16 41·14
——— apparent semidiam. ..	16 48·30		16 48·44		16 48·83	
——— ap. mot. fr. ⊙ in 60s ..	26 25		24·60		23·30	
Errors from instants assumed..	−13·35	+12·90	−23·18	+1·42	−12·12	+11·18
Times obtained	Ap. conj. 28d at 23h 5′ 30″·74		Begins 28d at 21h 58′ 57″·77		Ends 29d at 0h 11′ 30″·46	

The results in one view are as follow:

		D.	H.	′	″
Beginning of the eclipse at Greenwich, Nov.		28	21	58	57·77
Greatest obscuration			23	4	15·06
Apparent conjunction			23	5	30·74
End of the eclipse		29	0	11	30·46

Apparent time.

	D.	′	″
Digits eclipsed at greatest obscuration, on the north part of the sun's disc,	6	37	49·08

The moon will make the first impression on the sun's west limb at 35° 22′ 29″ from his zenith.

In your Magazine for April last, I observed a remark respecting the probability of an error of 6′ in the place of the moon's node as given in the Nautical Almanack for 1821, 1822, and 1823. This induced me to make a calculation of the lunar eclipse of the 6th instant; and in comparing the elements as obtained by interpolation from the Naut. Alm. with those found by calculation from the tables, I found the results differed very little. But on further consideration, it occurs to me that the error alluded to must be in the longitude of the node, as given for every 6th day, page 3, of each month: and it would appear that the computers of this part of the work have neglected a certain quantity which is applied to the supplement of the node in Burckhardt's tables, to make the equations additive.

I am, sir, yours respectfully,

Aberdeen, Feb. 15, 1822. GEO. INNES.

XLIII. *On the Combination of Silicium with Platina; and on its Presence in Steel.* *By* J. B. BOUSSINGAULT *.

IT was lately announced by M. Prechtel of Vienna, that he had succeeded in melting platina, in refractory crucibles, with an intense fire; and I therefore hoped (having access to the wind-furnace of the laboratory of the School of Mines, in which coke is used for fuel) to be able to accomplish the fusion of this metal: but the results were different from what I expected.

Of the Fusion of Platina.

One gramme of platina was placed in a plain earthen crucible; and a like quantity was put into a crucible lined with charcoal, and was covered with charcoal powder.

The two crucibles were set in a wind-furnance, and exposed for three hours to a very violent heat. (Under the same circumstances M. Leboulanger succeeded in fusing a perfect button of manganese.)

* From the *Annales de Chimie et de Physique.*

The platina in the plain crucible did not melt; it only acquired a greater lustre. That in the charcoal crucible was completely melted into a button.

These experiments were repeated several times, and always with similar results; and the metal was fused much more readily when covered with charcoal powder.

It was perceived, that the melted platina gained a small increase of weight in the process, showing its combination with some substance, which was naturally concluded to be charcoal, since it was every where in contact with this body.

The melted platina exhibits the following properties: Its aspect is greyish-white; it is with difficulty cut with a knife, does not easily yield to the file; its specific gravity is 20·5. In the cold it flattens a little under the hammer, but it soon cracks and presents a granulated fracture. When hammered at a cherry-red heat it becomes grained; at a very low red it slightly flattens, and then cracks. It does not in any degree alter its temper by heating and gradual cooling. Exposed to the blast of a forge-furnace, it was not even softened. As this method was not sufficient to drive off the supposed carbon in its composition, it was cemented for an hour with oxide of manganese; but the button of platina lost none of its properties, and remained equally refractory; and I then began to doubt of the presence of carbon, which I had taken for granted. It was, therefore, important to examine whether platina would, like iron, combine with charcoal by cementation. For this purpose I stratified slips of platina with powder of wood-charcoal in a crucible, which was heated very strongly for four hours, but to a degree short of the melting point of platina thus circumstanced. On examination, the platina was found to have lost part of its lustre, and its surface presented small inequalities, like blistered steel. Its specific gravity was from 17·5 to 18. It acquired in the process a considerable hardness, so as easily to scratch pure platina, and even iron, but not steel. Its hardness was not increased by quenching in cold water. It had gained by cementation, as well as by fusion, a small increase of weight. Perhaps this process might be of some use in the arts, either in cutlery, or particularly in gun-making, where the softness of common platina is complained of.

Examination of the melted and cemented Platina.

Eighty grammes of this platina were treated with aqua-regia. The solution was more difficult than with pure platina. No trace of carbonaceous residue appeared during solution; but, as it proceeded, there was observed a transparent jelly, which covered the bits of platina, and rendered the solution more difficult. By long digestion and much shaking, the whole, or nearly so, was dissolved;

dissolved: this was evaporated, and the dry salt redissolved in water, leaving a white powder behind.

This powder was then heated in a silver crucible with three parts of potash, in which it readily melted, and the alkaline mass easily and totally dissolved in water, with the exception of some minute fragments of platina separable by the filter. Sulphuric acid poured into the filtered fluid gave a white gelatinous precipitate, which was evidently silex. It is probable, therefore, that the wood-charcoal (which yields by combustion 2 or 3 per cent. of ash, chiefly siliceous) furnishes the silex that unites with the platina during the cementation, probably in the form of silicium, every circumstance being favourable for the reduction of the silex into its metallic basis. The silicium is not furnished by the crucible, for the cementation of the platina takes place equally well when a pretty large crucible is employed, and stuffed full of charcoal, with only a small cavity in the middle of it, to receive platina. The increase of weight thus acquired by the platina is very trifling. It is necessary not to use too much platina relatively to the quantity of charcoal, otherwise the fusion goes on very imperfectly, or not at all.

To ascertain further, whether the wood-charcoal furnished the silex, I repeated the experiment, using lamp-black instead of common charcoal; but the platina returned from the crucible unchanged, and quite ductile.

To judge of the quantity of silicium absorbed by the platina in the above-mentioned process, I took in one experiment exactly five grammes of platina, and after fusion the button weighed 5·025. One gramme of this button gave on analysis ·010 of silex. If the silex were in the state of earth in the metallic button, one gramme should have yielded only ·005, and therefore we must admit that it alloys with the platina in the state of silicium, and that it absorbs ·005 (or its own weight) of oxygen by solution in aqua-regia, whereby it passes into the state of silex. These are the proportions which I have assumed in calculating that of silicium as it enters into the composition of steel.

On Silicium in Steel.

The conversion of iron into steel is attributed to carbon alone; and this opinion, supported by the experiments of Monge, Berthollet, and Vandermonde, has been generally adopted by all chemists who have turned their attention to this subject. It is true that carbon is always found in steel; but another product, silex, which is as constantly obtained in the analysis of steel, and sometimes in as large a quantity as the carbon, has been usually considered as accidental. I have, therefore, expressly sought for the

the silex in the analysis which I have made of several of the products of the foundry of La Berardiere.

I dissolved the steel in sulphuric acid, diluted with six times its weight of water. The insoluble residue was then dried, weighed, and burned, and the proportion of carbon was inferred by the loss in burning. It deserves to be noticed that this carbonaceous residue takes fire long before the platina crucible is red-hot; sometimes even when it is no hotter than the hand can bear. The residue after combustion was then digested with dilute muriatic acid, which dissolved the metallic oxides, leaving the silex pure, which last was then calcined and weighed when warm.

In this procedure the estimate of carbon is far from being rigorously accurate, but my principal object was directed to that of the silex. Experiments were made with four different samples, namely, 1st. Iron (*de rive*); 2d. Cemented steel; 3d. Cast-steel; 4th. Steel from Monkland near Glasgow, made with Dannemora Swedish iron.

The products were as follow:

	Iron.	Carbon.	Silicium.	Mang. & Copper.
No. 1,	99·825	a trace	0·175	a trace
2,	99·325	0·450	0·225	ditto
3,	99·442	0·333	0·225	ditto
4,	99·375	0·500	0·125	ditto

It appears, therefore, that during the cementation of iron into steel, it absorbs a small quantity of silicium as well as carbon; but I state this with some doubt, as it requires a greater number of analyses with the same iron both before and after cementation.

The combination of iron with silicium was long ago hinted at by Clouet. He says expressly that iron combines with glass; and of all the experiments that could be imagined to prove the property possessed by silicium to convert iron into steel, none would be more conclusive than that of this eminent chemist; but such is the force of preconceived opinion, that he interpreted his result in favour of carbon.—His process was, to melt soft iron with a mixture of clay and chalk, and it turned out good cast-steel: and being satisfied that steel must contain carbon, he inferred that his product contained it, and explained its presence from the decomposition of the carbonic acid of the chalk by the iron at a high temperature, without ever ascertaining by analysis whether carbon was really present in his steel.

To be satisfied of this fact, I repeated Clouet's process, following with scrupulous accuracy the description which he has given in his report to the Institute. (*Journal des Mines* XVIII.) The

The iron which I employed was first assayed by digestion in dilute sulphuric acid, in which it dissolved without leaving any sensible quantity of residue.

The crucible was put into the forge at seven o'clock. At eight, the fusion being complete, I cast the metallic contents; the crucible having stood so well that it might have served a second time. Having thus obtained a quantity of Clouet's steel, I proceeded to examine its properties.

It yields to the file, and is forged with more difficulty than the steel of La Berardiere. It shows no spot after nitric acid has stood on its polished surface. It dissolved with difficulty in dilute sulphuric acid, preserving its metallic brightness all the time. The residue was very bulky, and proved to be silex quite pure and white, being in the proportion of 1·6 per cent. of the iron employed, 0·8 of silicium.

This steel, therefore, consists simply of 99·2 of iron, and 0·8 of silicium, without a particle of carbon: nevertheless the name of steel can hardly be denied to it, since it has the characteristic property of having its temper hardened by heating and sudden quenching in water. It may, therefore, be maintained that, for the conversion of iron into steel, silicium appears at least as essential as carbon, since we have none without the former; but we have one species without the carbon. Our knowledge on the subject, however, is too limited to enable us to deny the utility of carbon in steel, which perhaps is necessary to make it more easily wrought; and in fact all the kinds of steel that are employed contain carbon, whilst no use is made of that of Clouet.

Of Cast-Iron.

The fusibility of iron is shown by melting the metal in a Hessian crucible in a forge-heat. It may be questioned, however, whether the metal is pure iron.

Ten grammes of small nails were cut in pieces, half of them were dissolved in dilute sulphuric acid without leaving the smallest residue: the other half were melted in a Hessian crucible, yielding a well-fused and very brilliant button. This was more difficult to file and to forge than the iron which furnished it; like Clouet's steel, it preserved its metallic brilliancy during its solution in dilute acid; and it left behind a very white bulky residue of pure silex. The melted button was, therefore, composed of 99·46 per cent. of iron, and the silex obtained by solution was 1·08, being equal to 0·54 of silicium. This melted iron, therefore, has the greatest analogy with the cast-steel of Clouet: but in the latter case the clay and the chalk with which the iron is covered, form a siliceous envelope, in which the metal is kept immersed,

immersed, and which easily dissolves the oxide of iron formed by the decomposition of the silex, thereby facilitating the reduction. Whereas, when the iron is fused by itself, the silex can only be furnished by the crucible, to which it coheres with considerable force; and the oxide of iron, as it forms, soaks into the crucible, and serves to protect the earth from the contact of the metal; which is doubtless the cause why the conversion into steel cannot be completed without the presence of a glass.

We cannot, therefore, judge of the degree of heat required for the fusion of iron in a Hessian crucible, since it appears demonstrated that at a very high heat iron reduces silex, and combines with the silicium thus produced into a compound more fusible than iron *per se*. On the other hand, when platina is heated with silicium already formed, it unites with it into a more fusible compound; but if this metal does not melt by itself in a Hessian crucible, it is because it has so little affinity for oxygen, that it has not, like iron, the property of decomposing silex.

Though we cannot fix the degree of fusion of pure iron, any more than that of platina or of manganese, we may at least determine their relative fusibilities when in contact with charcoal or silicium, or both together; which, in a crucible lined with charcoal, is in the following order; namely, iron, platina, and manganese: and if we admit it to be probable that this is the real order of fusibility when they are pure, it will follow that manganese is a more refractory metal than platina.

XLIV. *Account of the Levelling taken from the Trigonometrical Station on Rumbles Moor and the Observatory, to the Canal, and ultimately to the Irish Sea; being a Continuation of the Article given in our last Number, p.* 130. *By* A CORRESPONDENT.

To Dr. Tilloch.

SIR, — THE usual method of measuring the fall of a declivity is by means of a telescopic level placed between two staves marked with feet and inches, with a little additional apparatus to enable the observer to raise or depress the cross wires to the nearest *inch* on the first erected staff, and also to alter the height of the one in advance until a particular inch is covered by the telescope (by which means the fractional parts of inches and the use of the sliding vanes may be avoided):—a more accurate method could not be devised. It must, however, be found extremely tedious in practice, and the more so in proportion to the abruptness of the descent. Wishing if possible to avoid any errors of an optical nature,

nature, the following instrument was prepared; but, from the marshy nature of the summit of Rumbles Moor, could not be rendered serviceable.

An inflexible deal rod, carrying at each end a thin steel plate about six inches square, and exactly ten feet asunder, being placed upon a perfectly horizontal plane, the brass plate of the large sector was attached to the rod in a vertical position, and the bubble of the index-level (at zero) adjusted to its mark. The instrument being placed upon two stands, erected upon the declivity of the moor, with their upper surfaces truly horizontal, the index when levelled would mark the angle of inclination, which with the constant radius of ten feet, and a table of natural sines, would give the fall from one stand to the other, from the summit to the base of the mountain. The brass plate with the divisions being perpendicular to the steel ends, and the stands on which they rest perfectly *level*, it follows that the former would be always in a vertical position, and that the bubble when once at its mark could not be displaced, however the position of the instrument might be varied.

To avoid the expense of a levelling instrument, the fall from Rumbles Moor to the canal at its base was determined with a four-inch theodolite, two staves about twenty inches in length, and a 100-feet tape.

When the *reaches* of the road would admit of it, the staves, commencing at the station, were placed nearly 200 feet asunder, and the theodolite erected and adjusted at the middle distance. The cross wires being fixed upon the centre of the white circles in the upper part of the dark-coloured staves (which to avoid parallax were described on thin paper, and had one common centre), the angles of elevation and depression were carefully read off to half minutes on the two-inch semicircle. The distances from the centres of the circles to the axis of the divided arch were next measured with the tape, and affixed to their respective angles.—With these data and a table of logarithmic *sines*, the difference of altitude of the staves is easily calculated.

A base trigonometrically determined (with favourable angles) to be 6719 feet, being measured with the tape, served to ascertain its error, as well as the trifling one of the scale with which in future it was from time to time compared.

This novel method of levelling is, I firmly believe, little inferior to the usual one in point of accuracy, and evidently preferable as far as regards convenience and dispatch. The staves may be placed (as is frequently required) in an oblique position, and the steeper the descent the greater the accuracy of the operation. It is scarcely necessary to remark, that the constant error of the instrument, the refraction, and the allowance for curvature,

curvature, being opposed to each other in quantities nearly equal, need not enter into the calculation.

In a second attempt to find the fall, the distances when even slopes presented themselves were sometimes as much as 2000 feet, and little attention was paid to the placing of the theodolite precisely at the medium distance.

The route adopted is upwards of four miles; yet the aggregate fall to various places, as determined by the two methods, rarely differed more than a foot. The mean gave 976 feet for the total perpendicular descent.*

Finally, A third or verification levelling was effected in nearly a direct line to the canal. The distances were repeatedly measured with tapes of different lengths, and the angles were taken with the horizon-sector. The last distance was found trigonometrically from two bases, all the three angles being observed. The sector was moreover taken to both stations, and the zenith distances reciprocally observed. The result is one foot less than the mean of the preceding essays.

420·5 ft.	0°	57′	20″	elev.	7·01	fall
646·0	3	11	50	dep.	36·00	
1362·5	2	34	50	elev.	61·34	
1361·8	3	18	15	dep.	78·49	
1576·4	4	48	32	elev.	132·15	
1577·6	3	17	47	dep.	90·83	
7841·0	4	6	4	dep.	562·00	
Height of instrum.				..	7·20	
Fall to canal at E. Morton					975·02	
Do. to basin at Liverpool					289·00	
Do. to low-water mark					54·00	
Height of Rumbles Moor above the Irish sea					1318·	

The last distance is horizontal; the others hypothenusal.

The altitude of the observatory was ascertained by a different and perhaps more satisfactory method. The elevations of three well defined (but inaccessible) objects, situated between the observatory and the canal, were carefully determined at a station on the banks of the latter. The depressions observed at the first-mentioned place gave the corresponding fall, and their sum the elevation of the observatory above the canal. The distances were found trigonometrically, from stations linked to the Ordnance survey, and were of course of accurate origin.

* When the intermediate object is *equi-distant* from the two stations, the refraction may be disregarded.

In

In a third attempt an intermediate accessible station was selected, and *all* the angles reciprocally observed. The mean of the several methods, as appears from the subjoined statement, is 275·3 feet.

	Sum of the two distances.	
By reciprocal observations	(12430 feet)	275·6
By object No. 1 ..	(17922 feet)	275·4
Do. .. 2 ..	(16591 feet)	275·5
Do. .. 3 ..	(15457 feet)	274·8
Mean		275·3
Canal above basin at Liverpool		63·0
Basin above low water		54·0
Height of observatory above the Irish sea ..		392·8
Difference of altitude of Rumbles Moor and the Observatory by levelling ..		926 feet
Do. by reciprocal observations with the horizon sector		927 feet.

As the measurements of the locks do not exactly correspond with the statement in Dr. Rees's Cyclopædia, I must, in candour, furnish the comparison.

	Dr. Rees. *Feet.*	Measured. *Ft.*	*In.*
	30	29	5
	279	279	11
	54¼	54	5
	67¾	68	4
Canal at the tunnel near Colne above the basin at Liverpool	431	432	1
Fall to E. Morton	150	142	11
	281	289	2
Fall to the river Aire	260	269	2
	21	20	

Mr. Priestley, in his plan of the canal, states the altitude at 22½ feet, and that of the basin at Liverpool at 56 feet 1⅞ inch above low water, or four feet more than it is given in the Cyclopædia.

At Pendle Hill, September 24, 1821 (half an hour after the sun had passed the meridian) the sea in the direction of a windmill east of Lytham (or W.S.W.) was observed to be depressed 53′ 54″. The instrument was 10 feet high, and the distance (by the map, corrected by the trigonometrical station at North Meals near the windmill) will be found to be 145,970 feet. Hence the ground at Pendle Hill was 1823 feet above the sea, to which add the fall to low water (?) for the correct altitude. It is stated at 1824 feet in the present list.

In calculating the heights of the *stations* the refraction was determined by the reciprocal observations, but in computing the altitudes of the other places in the list the zenith distances were INVARIABLY increased 1-15th of the arc, *minus* 25″, the error of the instrument.

When an object was observed only during the existence of the diurnal refraction, and the extremes were not marked, the only resource consisted in comparing it with a nearly contemporary observation of another object, of which the constant angle had been subsequently noted. The correction is nevertheless somewhat arbitrary; for the variation does not affect the whole of the observations either at the same time, or in the same degree.

To ensure greater accuracy to the altitudes of the *stations*, the following method was resorted to.

Rumbles Moor, as has been already demonstrated, is about 1318 feet high, and the altitude of any other object in its vicinity might be obtained by using the sector at a third station *equidistant* from the other two, without making any correction for refraction, or trival constant error of the instrument. When the distances are not precisely equal, allowance, it is true, must be made for refraction, &c. but great precision as to their value is not absolutely requisite to produce a correct result. It would however be possible to quote a solitary yet very marked exception, occasioned no doubt by an extraordinary state of the atmosphere in the direction of the object.

A few of the observations made on Pendle Beacon are adduced by way of illustration. (It is unnecessary to reduce the angles to the ground.)

Sep. 24, 1822. h. m.		Left Index. ′ ″	Rt. Ind. ′ ″	Therm.	Dist. in feet.	Arc. ′ ″	Feet.
10 50	AT Rum. Moor	dep. 24 53	25 10	48	102506	East 16 46	517·8 lower.
13 0	Do.	24 53	..	48½			
11 45	.. Gt. Wherns.	elev. 3 53	3 55	48½	124750	N.E. 20 27	474·8 higher.
11 20	.. Ingleboro.	elev. 8 12	8 12	49½	110820	N. 18 13	529·7 higher.
13 45	.. Pennigent	elev. 6 40	6 43	49	105492	N.N.E. 17 18	446 0 higher.

N.B. The sector reversed accurately at 60°. The refraction used is 1-15th—25″.

With these data and 1318 feet the altitude of Rumbles Moor we have 2310·6 for the elevation of Great Whernside, 2365·6 for that of Ingleborough; and 2281·8 for that of Pennigent.

In determining the distances in the survey the bases used were

Rumbles Moor to		Boulsworth Hill	68370	S.W.
Ditto	..	Pendle Hill	102506	W.
Ditto	..	Gt. Whernside	101114	N.N.W.
Ditto	..	Wakefield Spire	107336	S.E.

See *Trig. Survey, 3d volume.*

At

At Rumbles Moor and at the Observatory the principal angles were measured with a twelve-inch repeating circle of a peculiar construction, but not calculated to take vertical angles. The telescope, two feet in length, and furnished with cylindrical rings of equal diameter, and an excellent spirit-level, rests (in a pair of Ys) exactly over the upper part of the axis of the instrument, and serves to render the line of vision parallel to the plane of the divided circle. By fitting the telescope into an axis working in the Ys the angles can also be taken in azimuth, but what is gained in expedition and exemption from subsequent calculations is lost in point of accuracy. Frequent use of the instrument produces a shake in the centre, which renders the repeating property nearly worthless.

At Alfred Castle an eight-inch circle reading off to 15″, and fitted up as a transit with a low axis, and a fifteen-inch telescope, was made use of. One wire with a mark in the middle was found preferable to two.

At the other stations, viz. Symon Seat, Beamsley Rock, Carncliffe, Ilkley Wells, Chevin Beacon, the Bow, Eccles-hill Windmill, East Ardsley Church, Whitchurch, Jack-hill and Great Almias Cliff, the angles were measured by the four-inch theodolite. The divisions on silver read off to 1′. A plate and screw under the circle enable the observer to repeat the angles either in azimuth or in the plane of the three objects.

A box sextant reading off to 1′, was sometimes used in places difficult of access to take the third or verification angle.

To find the bearings of the different places the theodolite was fixed at the station on Rumbles Moor about the time of the summer solstice, and very carefully adjusted. The vernier being fixed to different *degrees* on the limb successively, the instants of the passage of the first and second limbs of the sun (then in the W.N.West) were carefully noted by the watch well regulated. The telescope was subsequently pointed at a distant well defined station over which the sun had passed during the preceding observations, and the angle repeatedly read off. The deduced azimuth differed so slightly from the one furnished by calculation from data in the Trigonometrical Survey, that either the one or the other might be used in computing the latitudes and longitudes without materially affecting the result.

The error of the watch was ascertained as well by sets of observations made with a ten-inch reflecting circle and an artificial horizon, as by the theodolite itself.

To find the altitude of any place contained in the annexed list as determined at any particular station where the sector was used, add to or subtract from the tabulated heights the feet and tenths affixed to their initials.

 The

The altitudes of the church towers (unless otherwise mentioned) are exclusive of the pinnacles and spires.

	Lat. N.			Long. W.			Altitude.
	°	'	"	°	'	"	
PENDLE HILL (P.H.)[1]	53	52	11	2	17	21	1824 feet
Boulsworth Hill[2] .. (*upright stone.*)	53	49	5	2	5	58	1697
Ingleborough[3] ..	54	10	4	2	13	18	2368
Pennigent[4] ..	54	10	56	2	14	22	2281
GREAT WHERNSIDE (G.W.)[5]	54	9	44	1	59	24	2309
SYMON SEAT (West) (S.S.)[6] *Hut.*	54	2	8	1	52	23	1593
Symon Seat (East)[7] ..	54	2	10	1	51	54	1585
Poxstones Moor[8] ..	54	2	27	1	50	24	1513
Carncliffe (?)[9] ..	54	1	22	1	52	59	1471
Roggan Hall[10] .. (*Ridge S. end.*)	54	1	5	1	49	30	1318
BEAMSLEY ROCK (B.R.)[11]	53	58	16	1	50	15	1310
Beamsley Beacon[12] .. (*Ground.*)	53	58	10	1	50	34	1286
Gaisegill[13]	53	58	44	1	48	40	1332

[1] A well known hill in Lancashire, near Clithero. The Beacon hillock is about eight feet higher. (By R.M. —1·6 foot. By S.S. —3·0. By G.W. +7·4. By B.R. —0·5.

[2] Near Colne in Lancashire. (By R.M. —5·8. By P.H. +1·8. By B.T. +3·7. By C.B. +0·3.)

[3] A majestic mountain three miles N.E. of Ingleton, near Settle. The old building on the west side is several feet higher. (By R.M. +2·4. By P.H. —5·2. By G.W. +6·5.)

[4] A steep mountain six miles N. of Settle. (By P.H.)

[5] Highest rock on the S.W. side of a huge shapeless mountain at the head of Netherdale, and three miles N.E. of Kettlewell in Craven. (Its altitude above the sea, as determined by the barometer, is stated in Dr. Whittaker's Hist. of Craven to be 1710½ feet!) A nameless mountain to the N.N.W. is still higher. (By R.M. —1·3. By B.R. —1·3. By S.S. +1·3.)

[6] A pile of rocks eight miles N.E. of Skipton, on East Barden Fell. The hut is about six feet higher than the rock on which it stands. (By B.R. —0·6. By R.M. +0·2. By A.C. +3·7.)

[7] A larger pile of rocks to the East. (By R.M. +2·2. By S.S. —2·3. By B.R. +0·1. By C.B. —0·1.)

[8] A rocky eminence 7378 feet to the East of S.S. (By B.R.)

[9] Some low rocks on East Barden Fell, and one mile S.W. of Symon Seat Hut. (By B.R +2·1 By S.S —2·1.)

[10] A white building for the accommodation of shooters, three miles N.E. of Bolton Abbey. (By R.M +1·2. By I.H. +2·3. By B.R. —0·7. By C.B. —2·4. By G.A.C —0.4.)

[11] On Blaeber Fell two miles S.E. of Bolton Abbey. (By R.M.)

[12] 427 yards W.S.W. of the rock. (By R.M. +0·8. By B.R. —0·8.)

[13] About 1¼ mile E.N.E. of the rock, and is the highest ground on Blaeber Fell (or Gawk-hill Ridge). A Roman road to Isurium passes over it. (By R.M. —0·3. By B.R. +0·3.)

Denton

	Lat. N.	Long. W.	Altitude.
Denton Church[1] .. *(excl. of Cupola.)*	53° 56′ 17″	1° 46′ 21″	402 feet.
JACK HILL (J.H.)[2] .. *(highest rock.)*	53 57 35	1 40 3	951
Little Almias Cliff[3] ..	53 58 2	1 38 23	837
GT. ALMIAS CLIFF (G.A.C.)[4]	53 56 16	1 35 10	716
Harley Hill[5] .. *(E.S.E. of Plantation.)*	53 58 59	1 33 25	596
Harewood Castle[6] .. *(S.W. Tower.)*	53 56 1	1 30 30	351
Inn on the Chevin[7] .. *(S. W. Chimney.)*	53 53 34	1 39 37	795
CHEVIN BEACON (C.B.)[8] *(Ground.)*	53 53 42	1 41 24	921
Otley Church[9] ..	53 55 0	1 41 20	279
Cow Rock[10] ..	53 55 6	1 47 42	860
Ilkley Church[11] ..	53 55 41	1 49 0	343
Ilkley Baths[12] .. *(Ground N.E.)*	53 55 6	1 48 50	682
Draughton Moor[13] ..	53 57 30	1 55 21	1074
Shode Bank Hill[14] ..	53 57 20	1 58 20	1223

[1] Two miles E. of Ilkley. (By B.T.)

[2] Four miles North of Otley, on the Washburn. (By R.M. +0·9. By B.R. −1·8. By S.S. +1·7. By A.C. +0·8. By P.B. (?)−12·0.)

[3] A pile of rocks 1¼ mile N.E. of Jack Hill. (By R.M. −0·1. By B.R. −1·4. By I.H. +0·6. By G.A.C. −0·4. By C.B. +1·4.)

[4] A picturesque group of immense rocks 4½ miles E.N.E. of Otley. By R.M. −0·1. By B.R. −1·6. By S.S. +3·4. By I.H. +1·1. By G.W. −9·8.)

[5] S.W. of Harrogate. (By B.R. +5·8. By G.A.C. −5·8.)

[6] Eight miles N. of Leeds. (By R.M. +1·4. By C.B. −0·8. By G.A.C. −0·6.)

[7] One mile and a quarter S.E. of Otley on the road to Leeds. (By B.R. and by A.C. the same.)

[8] One mile S. of Otley on the highest part of the hill. (By R.M. +1·2. By B.R. −0·7. By G.A.C. −1·5.)

[9] Ten miles N.W. of Leeds. (By B.R. +1·0. By G.A.C. −1·0.)

[10] One mile S.E. of Ilkley; a picturesque rock on Rumbles Moor 77 feet long, 58 feet high, and 42 broad. (By C.B.)

[11] Six miles W. of Otley. (By B.R.)

[12] One mile S. of Ilkley, on Rumbles Moor. The temperature of the spring is invariably 48° in summer. (By B.R.; the angles were reciprocally observed.)

[13] Two miles W. of Addingham, to the left of the now disused road to Skipton. (By R.M. +0·1. By C.B. −0·1.)

[14] Nearly two miles E.S.E. of Skipton, to the right of the old road to Addingham. It is the highest of several similarly shaped hills around it. (By B.R.)

Bolton

	Lat. N.	Long. W.	Altitude.
	° ′ ″	° ′ ″	
Bolton Abbey[1] ..	53 59 7	1 52 50	397 feet.
(*leaded ridge, E. end.*)			
Barden Fell, West side[2]	54 1 35	2 0 10	1663
(*highest rock.*)			
Burnsall Fell[3] ..	54 2 12	1 58 10	1505
(*Shooters-house, Ridge.*)			
Flasby Fell[4] ..	53 59 41	2 3 16	1170
York Minster[5] ..	53 57 48	1 4 34	253
(*Great square Tower.*)			
Brimham Crags[6] ..	54 4 59	1 43 1	990
(*Guide's house, Ridge.*)			
Michael Howe[7] ..	54 5 59	1 34 21	622
(*Spire.*)			
Whitchurch[8] ..	53 47 55	1 26 35	384
Braim Farm[9] ..	.. ..	.. ..	461
(*highest part.*)			
Trinity Church (*Fleece*)[10]	53 47 50	1 32 11	271
St. John's do. (*Vane*)	.. ..	.. ..	251
St. Paul's do. (*Cross*)	.. ..	.. ..	248
Potternewton Windmill[11]	53 49 30	1 32 28	420
(*Roof.*)			
ALFRED CASTLE (A.C.)[12]	53 50 33	1 32 53	489
(*highest part.*)			
Cookridge-hill[13] ..	53 51 22	1 36 16	645
(*Guide post.*)			

[1] Five miles and a half E.N.E. of Skipton. (By R.M. +0·1. By B.T.—0·1.)

[2] A lofty range of mountains, extending in a N. Easterly direction from Skipton nearly to Grassington. (By S.S. —1·2. By P.H. —1·6. By C.B. —1·1. By R.M. +2·0. By B.R. +0.1. By G.W. +1·6.)

[3] The northern termination of Barden Fell. (By R.M. +0·2. By B.T. —1·1. By S.S. +0·9.)

[4] A conical hill, three miles N.W. of Skipton. (By R.M. +1·0. By B.R. —4·6. By C.B. +3·7.)

[5] (A single observation from Rumbles Moor. Distance 1619·24 feet. The altitude is probably in defect.)

[6] Two miles N.E. of Paiteley Br. (By R.M.)

[7] One mile S. of Fountain's Abbey. (By R.M. —0·8. By G.A.C. +0·8.)

[8] A conspicuous Church Tower, four miles E. of Leeds, on the road to Selby. (By R.M. +0·7. By A.C. —0·3. By Observatory —0·4.)

[9] At Roundhay, four miles N.N.E. of Leeds. (By A.C. distance from Map.)

[10] At Leeds. (By Observatory.) N.B. The solar Eclipse, Sept. 7, 1820, commenced at 12h 11′ 30″ M.T. The calculated time is 12h 10′ 48½″.

[11] Two miles N.N.W. of Leeds. By A.C. +0·5. By Observatory —0·5.)

[12] A building 26 feet high, on Tunnilaw-hill, three miles N. of Leeds. (By Observatory.)

[13] Four miles and a half from Leeds, on the road to Otley. (By A.C. +0·7. By C.B. —0·7.)

Billing

		Lat. N.	Long. W.	Altitude.
Billing[1]	..	53° 51′ 20″	1° 39′ 51″	769 feet.
Sutton Crag[2]	..	.. ..	.. ..	1161
Eccleshill Windmill[3] (*Roof.*)	..	53 49 22	1 43 10	741
Calverley Church[4]	..	53 49 55	1 40 41	421
Wortley Windmill[5] (*Roof.*)	..	53 47 32	1 35 37	426
Armley Chapel[6] (*Belfry.*)	..	53 47 47	1 34 47	312
Wortley Chapel[7] (*Belfry.*)	..	53 47 20	1 34 58	326
Rothwell-haigh[8] (*Engine-House, Ridge.*)	..	53 45 30	1 29 25	309
East Ardsley Church[9]		53 43 29	1 32 15	489

A vast number of observations with the horizon sector are at present useless for want of the distances.

Comparison of Altitudes determined trigonometrically and by the Barometer.

	Trig.	Bar.	
Rumbles Moor and Ilkley Baths ..	636	611	−25
Do. and Canal	975	948	−27
Shode Bankhill and do.	880	848	−32
Pendle-hill and do.	1396	1362	−34
Observatory and Alfred Castle (*ground*)	72	72	0

The heights of the thermometer and of the barometer (a very ordinary portable one) were noted at short intervals, an hour or two before descending the mountain; and similar observations were repeated on reaching the inferior station. The fall or rise of the mercury during the half-hour elapsed in making the descent was thus ascertained, and the observations reduced to one

[1] Seven miles N.W of Leeds near Rawdon, commanding a beautiful and extensive prospect. By Observatory —1·4. (By A.C. +0·8. By C.B. —0·1. By B.T +1·1. By R.M. —0·6.)

[2] Six miles W.N.W. of Keighley, on the road to Colne. (By R.M.; distance from Map.)

[3] Two miles N.E. of Bradford. (By A.C. +1·0. By C.B. +0·3. By R.M. —1·4. By Observatory +2·0.)

[4] Six miles N.W. of Leeds, near the Canal. (By R.M. —4·4. By Observatory —0·7. By C.B. +2·3. By A.C. +1·5.)

[5] Near Leeds. (By Observatory +1·4. By A.C. +1·1. By C.B. +0·1. By R.M. —2·7.)

[6] Near Leeds. (By A.C. —0·6. By Observatory +0·6.)

[7] Near Leeds. (By A.C. +0·7. By Observatory —0·7.)

[8] Three miles S.S.E. of Leeds. (By A.C. —1·1. By Observatory +1·1.)

[9] N.W. of Wakefield. (By R.M.; an indifferent observation.)

particular

particular time. As the scale of the barometer bears examination, and as the formula (Dr. Maskelyne's) will scarcely be questioned, it is only in the specific gravity of the mercury, or in an erroneous estimate of the proportion of the area of the tube to that of the cistern ($\frac{1}{35}$), that we can look for the uniform discrepancies.

Comparison of the Altitudes given in the Trigonometrical Survey and in the present List (reduced to the ground).

	Table.	Trig. Survey.	Dif
Ingleborough	2368	2361 ..	7
Pennigent	2281	2270 ..	11
Great Whernside	2309	2263 ..	46
Rumbles Moor	1318	1308 ..	10
Pendle	*1824	1796 ..	28
Boulsworth	1692	1689 ..	3

The differences are in general very trivial; and may we not assign as a reason for the two marked exceptions, that the great theodolite was not used either at Pendle Hill or at Great Whernside, and that the refractions made use of in the calculations were greater than the reciprocal observations in the vicinity could warrant?

All the angular instruments employed in these operations were made and divided by the late Mr. James Allan.

☞ The author is respectfully informed, should the Journal of the Thermometric Indications at the summit and base of Rumbles Moor, which he states has been kept from the beginning of February, and will be discontinued on the 1st of April, present any interesting results, that I shall be happy to make room for it in the pages of the Phil. Mag. and Journal.—A. T.

XLV. *On Refraction.* *By* Joseph Reade, *M.D.*

[Continued from vol. lviii. p. 254.]

In my last communication I mentioned a simple, and I hope, in the opinion of men of science, a conclusive experiment against the commonly received doctrines of refraction. I shall now mention the following variation. Having procured a very clean cylindrical tumbler (fig. 7, Plate III.) with a flat bottom, about three inches diameter, I placed half-a-crown at the bottom, and holding it near a well lighted window, I poured in water by very small quantities at a time, my eye being on a plane: as soon as the object was entirely covered, a reflected image formed immediately over it, which rose with every addition of water: having poured

poured in to the height of about one inch, the image floated at the surface. I now covered the tumbler up to this surface with black cloth, and desired an assistant to throw in different coins, while I kept my eyes shut, each of which I described on again opening my eyes, by looking at their images floating on the surface of the water an inch above the coins, my eye being on a line with that surface, as thus represented: *a b* a tumbler filled to the height of one inch with clear water, and covered with black cloth; *c* a half-crown placed at the bottom; *d* the reflected image immediately over the coin, and seen by an eye at *e*.

Now, sir, I would beg leave to ask any person, not entirely blinded by prejudice, Is there not a reflected image formed perpendicularly over the piece of money, capable of being seen by an eye above, below, and on a line with the surface? Query,—Does this reflected image send rays, or rather, an image, to the spectator's eye? To see is to believe *. But, sir, in your last Journal there is something about the analogy between reflection and refraction: however, as no particular objections are brought against my opinions, I must think it a waste of time to answer vague and angry generalities. I am well aware, that my opinions on vision, light, and colours, are diametrically opposite to those of the schools, and entertain too high a respect for their professors not to believe that they will undergo a liberal and unprejudiced examination. If the gentleman be really serious in denying the evidence of his senses, he must come to particulars.

Now let us examine this experiment according to the received laws laid down in every elementary treatise on optics; and I contend that no refraction or bending of the rays can possibly take place at *d*, for the rays *c d* enter the air perpendicular to the plane surface of the water; consequently they must pass on without any refraction. Mr. Harris has a figure (see fig. 8.) illustrating refraction at plane surfaces. Suppose the vessel empty, B K its side, and Q the object at the bottom; if the eye be at *e*, the object will be hid by the side B K; but by filling the vessel with water, it will become visible, and be seen at *q*. The ray Q B being refracted into B *e*. Mr. Harris speaks as if an image were formed in the body of the water at *q*. For the purpose of making the rays of light enter the air in an oblique direction, mathematicians have made them to diverge from the point Q. On the contrary, we find by direct experiment, that an image of the half-crown is formed over the piece of money, which could not be the case were the rays diverged: that it is a reflected and not a re-

* If the rays *c d* are refracted in the direction *e*, the rays *e* should be refracted in the contrary direction *d c*; and an eye under the water at *c* should perceive an object at *e*, which is impossible; for then the sine of incidence would be equal to a radius.

a refracted image we see, is evident from our being able to see it in every direction floating on the surface of the water: if refracted, we could only see it in the direction of the refracted rays. When the eye is placed immediately over the half-crown, looking down into the water, we see the image, not the piece of money, one-fourth nearer to the eye: here there can be no refraction, as the rays coming to the eye must be at right angles to the surface of the water: here there is no angle of incidence; no angle of refraction; no ratio of 3 to 4. In fact, this simple experiment rebels against almost all the laws of optics. Snellius was the first who supposed he discovered a constant ratio in refraction; he used the secants of the complements instead of the sines used by the celebrated Des Cartes. As his doctrines are founded on this experiment, I think it necessary to make a few observations.

Supposing the surface of the water to be A B (fig. 9), and an object under it at D, which to the eye at F appeared as it were in the line T C. He produced T C till it met in G with the perpendicular D A to the surface A B. Then he argued, that the image of the object D appeared at G, and that C D was to C G in a certain given ratio as 4 to 3 in water.

The following objections may be made: 1. The images can be seen by an eye at B on a plane with the surface of the water. 2. This image can be perceived in every direction above, below, and on a plane with the surface of the water, which could not be the case with a refracted image. 3. There is no reason whatsoever that the ray D C should be refracted in the diagonal at plane surfaces, except for the purpose of supporting the theory. On the contrary, there is every reason to prove that the rays move parallel, for the image is perceived at A immediately over the piece of coin. An eye at A looking down into the tumbler sees the piece of money one-fourth nearer. Here, according to opticians, the rays are not refracted; yet they cannot deny that the piece of money appears nearer the eye, and somewhat magnified. If it were the object and not an image they saw, it would appear at the same distance as in air. It is agreed on all hands, that every refracting surface forms a reflected image; why resort to any other means? I shall now proceed to extend this experiment to a medium terminated by two plane surfaces inclined to one another, such as an equilateral prism.

Having placed a sovereign under the plane of a prism (fig. 10.) resting on the table, I found that two reflected and not refracted images were formed in each plane, as represented in the following figure. *a* The sovereign placed under the plane *d c* of an equilateral prism, forms an image at *a*; which image sends images to *b* and *f*. That these are reflected and not refracted images, is so evident as scarcely to require remark. According to

to the present theory, two images could not possibly be formed by refraction at *b* and *f*; for *a* being at right angles to the plane *d c*, the rays should suffer no refraction, but proceed on to the vertex. The very same mistake which induced optical writers to suppose from analogy that rays converged in the body of a convex lens, made them also suppose that rays were turned in the body of a prism to the thickest part as well within as without the medium. Let us now examine this experiment according to the present laws of optics.

Let the angle C A I (fig. 11) be a right angle; then the whole refraction is at C; and in this case, D C A : A C D :: $m : m-n$. Also, since the right angle D C I is equal to the sum of the two A C I, A I C, take away the common angle A C I, and the remaining angles D C A, A I C are equal. Consequently A I C : A C I :: $m : m-n$. Now I would beg leave to ask, Does any light in this experiment pass through the plane Y Z? The ray Q A is undoubtedly turned to the thickest part of the prism; not, as Newton and his followers suppose, from any principle of attraction, but simply because it strikes the plane I Z obliquely, and there forms an image, which moves downwards. Let us vary this experiment. I placed the plane of the prism on a small hole, cut in a large sheet of pasteboard, and perceived two images of the hole formed in the planes, as already described with the sovereign. I now removed this sheet of pasteboard with the prism into the sun-beams, as represented (see fig. 12), and found that the rays passed through both planes S. The sun passes through a hole in the pasteboard, and, striking the plane A B perpendicularly, forms an image at *d*, which image sends rays to form other images at *f* and *g*. Here we have two spectra at *f* and *g*, the one ascending the other descending in consequence of striking the planes obliquely. In this experiment opticians are necessarily obliged to relinquish one of their favourite laws, "that rays striking at right angles to plane surfaces suffer no refraction; for it cannot possibly be denied, 1st, that the rays strike the plane A B at right angles; and 2dly, that the rays diverge: otherwise they could not come through the planes A C and B C. Are the rays refracted in opposite directions? or are they attracted and repelled in opposite directions? But if we admit that an image is formed at *d*, we can easily account for the two reflections at *f* and *g*. Had Sir Isaac Newton been acquainted with the formation of two spectra (and I cannot but express surprise that he was not), he never could have maintained the doctrines he did. Here I cannot but notice a curious fact in regard to the prism, although not immediately connected with the doctrine of refraction. When the sun-beams are passed through the lower refracting angle, as it is called, on emergence they ascend

and form a beautiful spectrum on the opposite wall, orange at the bottom, violet at the top, with intermediate colours: but on looking through the same refracting angle at the hole in the pasteboard or window-shutter, the experimenter is surprised to find all the colours reversed, violet at the bottom, orange at the top. Newton must have had very defective eyes, or must have been very inattentive, entirely to have overlooked this interesting fact; for we often find him in the Optics looking at the hole through the prism, yet never mentioning it. I shall explain this phenomenon in my treatise on Vision, with which it is intimately connected; and shall merely remark, that the rays forming the spectrum have nothing to do with vision-making images. "Then," says Newton, "I looked through the prism upon the hole. In this situation, viewing through the said hole, I observed the length of its refracted image to be many times greater than its breadth, and that the most refracted part thereof appeared violet, the least refracted red, the middle parts blue, green and yellow in order. The same thing happened when I removed the prism out of the sun's light, and looked through it upon the hole shining by the light of the clouds beyond it; and yet, if the refractions were done regularly, according to one certain proportion of the sines of incidence and refraction, as is vulgarly supposed, the refracted image ought to have appeared round." Here Newton's attention seems to have been so completely absorbed with preconceived opinions, that he never noticed the colours being reversed; and consequently, that the image he saw on the plane of the prism and that on the opposite wall were distinct and different, bearing no analogy whatever. On looking at the hole in the window-shutter through the lower refracting angle, we are obliged to direct the optic axis on a line with the ground, and then see a reflected and not a refracted image painted on the prismatic plane.

As I have shown in the first volume of the Experimental Outlines for a new Theory of Vision, Light, and Colours, p. 48, that Newton never separated what he calls white light into seven coloured rays, I think it perfectly unnecessary to speak of their different refrangibilities: any fluid passing through a resisting medium obliquely must be lengthened; and I have shown that a straight stick, when viewed through the prism, is curved; therefore it is not surprising that the image of the hole should be oblong, not circular, and bounded by two semicircular ends.

Here I think it necessary to mention, that when writing the Outlines I had not made the first experiment mentioned in this paper, and therefore believed in the theory of refractions. The next experiment on which the theory of refraction seems to rest, is the following: "Take an empty vessel, such as a basin, and all along

along the diameter of its bottom fix little marks at a small distance from one another; then, through a small hole in the window-shutter of a dark chamber, let in a beam of the sun's light: where the beam falls upon the floor, place your basin so that its marked diameter may point towards the window, and that the beam of light may fall on the mark that is most distant from the window: this done, fill the basin with water, and you will observe that the beam which before fell upon the most distant mark, will now, by the refractive power of the water, be turned out of its straight course, and fall two, three, or more inches nearer the centre of the basin." The fallacy of this experiment can easily be explained on the same principle as the first. I shall merely remark, that when the water is thrown in, we do not see the marks at the bottom of the basin, but reflected images of those marks floating on the water; and also the beam of light, when falling obliquely on the surface of the water, must cause a reflected image, such as an oar would. Therefore, any conclusions drawn from such an experiment must prove erroneous.

A very simple experiment may be made in the following manner: Cut a square piece of white paper about the size of a half-crown, and let it be dipped in a tumbler of clear water: on looking at it, it appears as if split into two papers, giving a simple but conclusive illustration of these reflected images.

I shall now say a few words on refraction through concave and convex lenses; nor do I see much occasion to enlarge on this part of my subject, having already in my paper on Vision, published in one of your former Journals, shown that the cornea and not the retina is the true and only seat of vision, and that the mind receives its ideas from minute images painted thereon, and not from any crooked refractions forming imaginary images in the air. Indeed, a person consulting optical writers, and referring to their figures explanatory of telescopes with four lenses, must suppose Nature, instead of being simple and uniform in her operations, to be fond of all manner of twistings and turnings. At the object-glass the rays get the first twist; two more at the medium-glasses; a fourth at the eye-glass; a fifth at the cornea; a sixth at the crystalline lens; a seventh at the vitreous humour; and, if it were necessary, a dexterous optician may twist it round his finger. Newton and De Domenis have done nearly as much with their two reflections and two refractions in the rainbow.

For the experiments with lenses, it is necessary to procure a glass globe about three inches in diameter, the bull's eye of a magic-lantern, and a concavo-concave lens. Having pasted a piece of black cloth in the shape of the letter T on a pane of glass at the window, I requested an assistant, when seated opposite, to look steadfastly at it: on now looking into his pupil, I perceived

ceived a beautiful reflected image of the letter T. I now placed the bull's eye immediately in front, and then perceived this image to be considerably magnified in all its dimensions and surrounded with colours: he said he saw exactly similar to the reflection on his cornea. I would now beg leave to ask, Did this gentleman perceive the letter T by a reflected or refracted image? On removing the bull's eye to yet a greater distance from the pupil, I distinctly perceived two reflected images, the one erect, the other inverted. Again I would ask, Is it possible by refraction to produce in the focus of a lens both an erect and an inverted image at one and the same time? That we see by means of reflected and not refracted images, is therefore evident. This experiment is easily repeated with the glass globe instead of an assistant's eye. On a sheet of white paper write the letter T, and hold over it the bull's eye: when close to the paper, the letter is considerably magnified: on bringing it somewhat nearer to the eye, two inverted and coloured images are perceived to float on the posterior surface. On now giving a circular motion to the lens, these reflected images, in revolving round the erect one, become inverted or erect; when at the top and bottom they are inverted; when at the sides erect; for which phænomenon I am as yet unable to account. At yet a greater distance these two images form a circular appearance, margined on the inside by orange rays, and at length coalescing form one inverted image, which floats around the erect image with each revolution, without change. When we look at an object, its picture is painted on the cornea, and thence converges to the sensorium in the same manner as with the other senses. By placing a concave lens before the eye, this reflected image is diminished; by placing a convex one it is magnified. A short-sighted person sees objects large and confused when at a distance; a concave lens obviates this defect, by painting a small and well-defined image close to the eyes; for a near-sighted person can read small print when near without glasses. In old age the humours become decayed and turbid, and the corneal image is not sufficiently strong to make an impression on the retina, the principal nerve of the eye. Therefore a convex lens is necessary for the purpose of forming a magnified image closer to the eye, and also for the purpose of illuminating that image and throwing a greater quantity of light into the eye. Any person may make himself near-sighted either by constantly examining near and small objects, or by the wearing concave glasses; for by these means the eye becomes accustomed to the strong stimulus of rays from near objects, or from the images near the eyes. In a similar manner, a person may make himself deaf, by constantly accustoming the ear to intense noises, such as the roar of cannon, &c.

Mr.

Mr. Ware has written an excellent paper on the use and abuse of glasses. Perhaps it may be objected to the first experiment of this paper, that the piece of money radiated light as if from a centre or focus. To obviate which, I varied the experiment in the following manner: I first placed the piece of money at the bottom of the tumbler, and then placed immediately on it a concavo-concave lens; on filling in the water, I found the image formed, as already represented. I now placed a plano-convex lens over it, with the same results: here the rays were reflected to a focus, and consequently they could not answer for a refracted image.

The theory of refraction and the retinal theory of vision are so intimately and inseparably united, that the one cannot exist without the other. I therefore would request Mr. Stark to read my paper on Vision, published in a former Journal. If I have expressed myself with too much confidence, I must express my regret, and hope the learned and candid reader (for learning and candour generally go hand in hand) may attribute it to haste, perhaps not unaccompanied by a feeling of resentment at prejudice and critical neglect. But, sir, I am now happy to see that my opinions are daily gaining ground, and sanctioned by men of the first-rate abilities. I am certain both Mr. Stark and myself have one and the same object in view, *the discovery of truth.* I therefore shall endeavour, as far as lies in my power, to answer any particular objections, but must decline a metaphysical controversy on the nature of light; especially as the theory of Newton or that of Des Cartes would equally answer for experimental inquiry. *Disputatio torquet homines,* says Cicero; and impressed with a high respect for that great orator, I would wish to avoid it.

Epicurus thought that vision was produced by a continual succession of material images sent to the eye, which at their first emission from the object are large, decreasing continually the further they go, till they arrive at such a smallness as will permit them to enter the eye. That images are sent off from bodies, can easily be shown. And if I have shown that the rays of light coming from all points of an object, and meeting again at the focus, do not make a picture of the object on any white body interposed, then we have no other alternative than to go back to Democritus and Lucretius.

I remain, sir, your obedient servant,

Cork, Feb. 26, 1822. JOSEPH READE, M.D.

XLVI. *An*

XLVI. *An Account of some Experiments on the Action of Iodine on volatile and fixed Oils, &c.* By EDMUND DAVY, Esq. *Professor of Chemistry and Secretary to the Royal Cork Institution.*

To Dr. Tilloch.

DEAR SIR,—I BEG to send you for insertion in your very useful Journal, an Account of some Experiments I have made on the Action of Iodine on volatile and fixed Oils, &c. With sincere good wishes for your health and happiness,

I remain, dear Sir,

With great respect, yours very truly,

EDMUND DAVY.

Being lately engaged in making some experiments with iodine, I was led to try its action on different volatile and fixed oils, &c. The results I obtained are, I presume, novel; and a brief account of them will make some addition to our present knowledge of the agencies of this singular substance.

Action of Iodine on Oil of Turpentine.

When a small portion of iodine is brought in contact with a few drops of turpentine, a violent action takes place, considerable heat is generated, and part of the iodine rises in vapour. In one instance, when I put less than a grain of iodine into a small curved tube, and poured a little turpentine on it, the heat produced was very sensible to the hand. In another case, when I added about ten drops of turpentine to about a grain and half of iodine, in a small phial, the action was very violent; a portion of the turpentine appeared to be decomposed, it became tenacious, adhered to the glass, and was of a dark olive-brown colour. Turpentine is a very good solvent of iodine, and dissolves a considerable quantity of it with much greater facility than alcohol does. When iodine is put into turpentine, a hissing noise is produced, the iodine quickly dissolves, and forms a solution of a reddish yellow colour, which, when very concentrated, is dark yellowish-brown. This solution is not affected by water, or by the mineral acids when diluted, or by the greater number of metallic salts. The nitrates of silver and mercury, however, decompose it, and the iodes of silver and mercury are formed. By dissolving iodine, turpentine, to a certain extent, loses its characteristic odour and volatility; the solution, when weak, does not affect vegetable colours, or tarnish polished silver; but when strong, it gives a reddish-brown tint to litmus, and a dull yellow to silver and tin. It stains linen yellow, and gives to starch a slight yellowish tint. Rectified sulphuric ether and alcohol combine

combine with the solution of iodine in turpentine, and form homogeneous fluids. Phosphorus soon destroys the colour of the solution of iodine in turpentine, the fluid acquires the odour of phosphorus, and reddens litmus paper; probably in this case the hydroiodic acid is formed. Alkalies also readily change the colour of solution of iodine in turpentine, and form yellowish saponaceous substances. When heat is applied to the solution of iodine in turpentine, a portion of the oil distills over unaltered; but as the solution becomes more concentrated, a dense yellowish-brown oil rises, which holds the iodine in solution.

The affinity of turpentine for iodine is much greater than that of water; hence turpentine readily separates iodine from its solution in water. This effect is immediately produced by merely agitating an aqueous solution of iodine in contact with turpentine; the water becomes colourless, and the turpentine assumes a reddish colour. In this way, an aqueous solution of iodine made above twelve months since, was immediately decomposed by turpentine. A piece of cork, also, after being acted upon by iodine for several months, so as to become soft and of a dark-brown colour, yielded in water a solution of iodine of a brownish yellow colour, which by agitation with turpentine became colourless, and at the same time the oil acquired a fine red colour. Turpentine, also, separates iodine from its aqueous solution, in cases when the mineral acids and a number of metallic salts are present; as the sulphuric, nitric and muriatic acids, the sulphate of zinc, muriate of platinum, nitrate of nickel, &c.

The property of separating iodine from its solution in water, ether possesses in common with turpentine. When chlorine is passed through a solution of iodine in turpentine, the colour of the solution gradually disappears. The iode of chlorine acts strongly on turpentine, and readily dissolves in it. I put about half a grain of iodine into a platinum spoon, and introduced it into a bottle of chlorine; the iodine melted, and readily formed the yellow iode of chlorine. I then poured a little turpentine into the spoon, when a violent action took place; the iode was partially decomposed, and a portion of its iodine rose in vapour; the remainder of the iode dissolved easily in turpentine, and formed a solution of a red colour, which, on being exposed to the action of the solar rays for a short time, became colourless, but did not affect litmus paper. I witnessed an interesting result on submitting the red solution of the iode of chlorine in turpentine to the action of chlorine. A platinum spoon being filled with this solution, was put into a bottle of chlorine; it presently began to boil, its colour disappeared, and the fluid burst into flame; a black carbonaceous matter, arising from the decomposition of the turpentine, deposited itself on the sides of the bottle. Being

desirous of ascertaining how far the iodine in the compound was connected with those effects, I filled the spoon with turpentine and put it into a fresh bottle of chlorine, when ebullition immediately took place, and was succeeded by the inflammation and decomposition of the oil.

2. *Action of Iodine on other volatile and fixed Oils, &c.*

The effects of iodine on oil of lavender are similar to those already noticed respecting turpentine. When iodine is brought in contact with the oil of lavender, a strong action takes place, heat is evolved, and a dark reddish-yellow solution is formed. Analogous results are afforded with iodine and the oils of caraway, peppermint, and origanum; but the action of iodine on these oils is more feeble than on those of turpentine and lavender, and it is stronger on the oil of caraway, than on the oils of peppermint and origanum. Oil of amber acts very feebly on iodine, and a solution of a reddish-yellow colour is slowly formed. Iodine is soluble in naphtha, and to a certain extent in olive oil and oil of ivy.

Fixed vegetable oils and animal oils have very little action on iodine. When put into rape oil, iodine does not dissolve; it becomes brown by a gentle heat, and acts slightly on the oil. The effects of hemp, linseed, olive, and castor oils, are very similar to those of rape oil. Those oils in general separate iodine from its solution in water, but the action of iodine upon them, and also upon spermaceti and pilchard oils, is very slight.

Iodine readily combines with camphor by a gentle heat, and a dark-brown soft solid compound is formed, which is deliquescent, soluble in water, but more soluble in alcohol or turpentine. When turpentine is added to the aqueous solution of iodine and camphor, it separates the compound and leaves the water colourless. On adding alcohol, the camphor is separated, whilst the iodine remains dissolved in the turpentine.

Resin unites with iodine by a gentle heat, and a dark brown compound is formed, which is soluble in alcohol. Turpentine separates the iodine, and water the resin.

3. *Observations, &c.*

From the foregoing experiments, &c. it seems that iodine exerts a strong action on volatile oils, and especially upon turpentine and lavender; but on fixed oils its effects are much less considerable. In general, both the volatile and fixed oils separate iodine from its solution in water. The action of iodine on volatile and fixed oils resembles that of chlorine on these bodies, a circumstance which serves to extend the analogies which Sir Humphry Davy has traced between iodine and chlorine in their

their chemical agencies *. As oil of turpentine separates iodine from its solution in water, and in cases when acids and a number of metallic salts are present, it may, in many instances, afford a useful test to detect the presence of iodine, or be employed as a means of separating it in a fluid form from other substances with which it may exist in solution. The nitrates of silver and mercury seem to offer the best means of detecting and separating iodine from its solution in turpentine; the iode of silver is of a paler and duller yellow colour than that of mercury. Polished silver, which Sir H. Davy found to be one of the best tests of the presence of iodine in compounds dissolved in water †, does not furnish satisfactory indications of its presence in turpentine, especially when it exists only in minute quantity. Except in cases when the fixed alkalies and ammonia are present in excess, starch seems in general to be a very delicate and unexceptionable test of the presence of iodine; but when added to a solution of iodine in turpentine, it merely acquires a yellow tint. The addition of starch to a solution of iodine in water, alcohol, &c. occasions, as is well known, the immediate formation of the purple compound of starch and iodine. But if starch in its common state of dryness be pulverized and mixed with iodine in small proportion, a very peculiar effect will take place, which I have not seen any where noticed. The mixture, at first, is of a grayish colour; but in a little time it acquires a faint purple tint, which gradually becomes deeper and deeper, till it appears almost black. These changes are probably connected with the absorption of moisture from the atmosphere; for if water be added to the above mixture, the purple compound will be directly produced. The agency of water or moisture seems to be necessary to the formation of the purple compound of iodine and starch, as may, I think, be deduced from the following experiments: I put some iodine into a small tube, and nearly filled it with starch in powder, which had been well dried: no apparent effect took place; the tube was gently heated so as to raise the iodine in vapour, and the starch was agitated. The same process was again repeated, but the starch merely assumed a light-brown colour. On exposing it to the atmosphere it slowly acquired a purple tint, and when moistened with water, or placed on wet paper, it immediately became of a bright purple colour.

Royal Cork Institution, March 11, 1822.

* Phil. Trans. 1814.
† Ibid.

XLVII. *On the early Blowing of Plants during the present Winter.* By Dr. THOMAS FORSTER, F.L.S. &c. &c.

To Dr. Tilloch.

SIR,—I PROCEED to send you an account of the unseasonable florescence of many plants this winter and spring, as I promised in my last paper on the Peculiarities of the Weather.

On the first of December a considerable number of plants belonging to the æstival and autumnal Floras remained in blow: among others may be reckoned the *Chrysanthemum coronarium*, *Scabiosa atropurpurea*, *Papaver Rhœas*, *P. somniferum*, and many varieties of Stocks. The following plants, however, came into flower after the first of December, and they opened their blossoms according to the dates subjoined.

	December	2.	*Helleborus hyemalis.*
		4.	*Papaver Cambricum.*
		4.	*Adonis autumnalis.*
		4.	*Tussilago fragrans.*
		9.	*Primula Veris.*
		9.	*Primula elatior.*
		15.	*Vinca major.*
		16.	*Viburnum Tinus.*
		24.	*Bellis perennis.*
1822.	January	4.	*Primulæ Polyanthi varii.*
		19.	*Lamium purpureum.*
		28.	*Primula vulgaris.*
		29.	*Galanthus nivalis.*
	February	6.	*Anemone hepatica.*
		6.	*Tussilago alba.*
		8.	*Crocus vernus.*
		19.	*Scilla Peruviana.*
		24.	*Anemone hortensis.*
		24.	*Daphne Mezereon.*
		24.	*Narcissus Romanus.*
		24.	*Narcissus papyraceus.*
		24.	*Viola tricolor.*
		25.	*Hyacinthus orientalis.*
		25.	*Narcissus Tazetta flava.*
	March	1.	*Leontodon Taraxacum.*
		4.	*Ficaria verna.*
		4.	*Tussilago Farfara.*
		5.	*Hyacinthus Botryoides.*
		5.	*Scilla amœna.*
		5.	*Narcissus Pseudonarcissus.*
		9.	*Narcissus lætus.*

9. *Calendula*

March 9. *Calendula officinalis.*
9. *Tussilago hybrida.*
10. *Viola Tunbrigiensis.*

I shall like to see the calendars of Flora kept by any of your correspondents in other parts of England, if they will be so obliging as to communicate them. I remain, &c.

T. FORSTER.*

* In our last Number, p. 154, last line, for J. Forster read T. Forster.—EDIT.

XLVIII. *Notices respecting New Books.*

Recent Publications.

Tables Astronomiques, publiées par le Bureau des Longitudes de France, contenant les Tables de Jupiter, *de* Saturne, *et d'*Uranus, *construites d'après la Theorie de la Mécanique Céleste; par* M. A. BOUVARD. 4to. pp. 138. Paris, 1821.

In the year 1808, M. Bouvard, who is well known as an indefatigable observer and calculator, constructed tables of Jupiter and Saturn, founded on the system of gravity, and on the several observed oppositions, from 1747 to 1804. Not long after the impression of these tables, M. de Laplace discovered an error in the analytical part of the process used in the construction, which influenced the values of the elliptic elements, and consequently the tables would not long continue to accord with observation.

Undaunted by this vexatious occurrence, M. Bouvard recommenced his labours, and in the *Connaissance des Tems* of the year 1816 published corrected elements of Jupiter; in the formation of which were employed all the observed oppositions and *quadratures* down to 1814. The elements of Saturn, in like manner corrected, appeared in the volume for 1818; and those of Uranus were promised.

In the present volume are comprised tables of the three planets constructed according to the decimal division of the circle (as were those of 1808); with an introduction detailing the formulæ as numerically expounded, and a comparison of the tables with the places determined by observation.

With regard to the tables of Uranus, two distinct sets of observations were to be regarded; the one comprising those made by accident, while its existence as a planet was unknown; and the other comprehending the observations from 1781 to the present time. Much industry had been employed by Bouvard, as also by Delambre, Burckhardt and others, to detect observations of this planet, as a fixed star, and the number hitherto ascertained amounts

amounts to twenty; viz. six by Flamsteed, one by Bradley, one by Mayer, and twelve by Lemonnier. It was very natural for M. Bouvard to combine the ancient and modern series, in deducing the planetary elements. Having done so, and compared his new tables with the observations, he found the ancient ones agreed but indifferently, while among the modern ones there was a regularly varying difference alternately positive and negative. These differences were so great, that it was impossible to attribute them either to the modern observations, or to the theory; and the care with which the calculations had been made, precluded the idea of assigning as the cause of the errors the omission of any important term. M. Bouvard was therefore obliged to reject the ancient observations, and to construct his tables anew, according to the modern determinations solely; which now extend to nearly one half of the planetary period. The present tables correspond very exactly with the last-mentioned places, none of the comparisons giving a difference of 10″; but the ancient observations are represented with much less exactness. Flamsteed's exhibit errors of from +41″ to +62″, and those of the other observers give from −14″ to −70″.

M. Bouvard leaves it to be ascertained hereafter, whether the above discordances are to be assigned to a want of exactness in the old observers and their instruments, as he himself believes; or whether they depend on some unknown cause of planetary perturbation.

To show the progress of modern science, we will call the attention of our readers to the tables published by Professor Vince in 1808, comprising the most exact ones then extant of the sun, moon, planets, and satellites. Since that period there have appeared the tables of Venus, by Reboul; of the Moon, by Burckhardt; of Jupiter's satellites, by Delambre; and of the three great planets, by Bouvard. Besides which, the tables of the Sun have been revised by Burckhardt, although, from the smallness of the corrections discovered by him, it has not been considered necessary to reconstruct the tables. So that Professor Vince's work is become obsolete, in the short space of 13 years, except as to Mercury and Mars, of which planets new tables may be expected from the hand of M. Burckhardt, according to an intimation given in the *Conn. des Tems* for 1816.

The First Volume of the Memoirs of the Astronomical Society of London, has appeared too late in the month to allow us to do more than merely notice its contents. In addition to the Address, Regulations and First Report of the Council of the Society, it contains the following interesting papers:

I. An Account of the Repeating Circle, and of the Altitude and

and Azimuth Instrument; describing their different Constructions, the Manner of performing their principal Adjustments, and how to make Observations with them; together with a Comparison of their respective Advantages. By Edward Troughton, Esq., F.R.S., and Member of the American Philosophical Society.—II. The Description of a Repeating Instrument upon a new Construction. By G. Dollond, Esq. F.R.S.—III. On a Method of fixing a Transit Instrument exactly in the Meridian. By F. Baily, Esq. F.R.S. and L.S.—IV. On the doubly-refracting Property of Rock Crystal, considered as a Principle of Micrometrical Measurements, when applied to a Telescope. By the Rev. W. Pearson, LL.D. F.R.S. and Treasurer of this Society.—V. On the Construction and Use of a Micrometrical Eyepiece of a Telescope. By the Rev. W. Pearson, LL.D. F.R.S. and Treasurer of this Society.—VI. On the Construction of a new Position-Micrometer, depending on the doubly-refractive Power of Rock Crystal. By the Rev. W. Pearson, LL.D. F.R.S. and Treasurer of this Society.—VII. Observations on the best Mode of examining the double or compound Stars; together with a Catalogue of those whose Places have been identified. By James South, Esq. F.R.S. and L.S. Honorary Member of the Cambridge Philosophical Society, &c.—VIII. On the new Meridian Circle at Gottingen. Communicated by Professor Gauss, in a Letter to the Foreign Secretary.—IX. On the Solar Eclipse which took Place on September 7, 1820. By F. Baily, Esq. F.R.S. and L.S.—X. On the Solar Eclipse which took Place on September 7, 1820. Communicated in a Letter to J. F. W. Herschel, Esq., Foreign Sebretary, from Professor Moll of Utrecht. —XI. On the Comet discovered in the Constellation Pegasus in 1821. Communicated in a Letter to J. F. W. Herschel, Esq., Foreign Secretary, from M. Nicollet of Paris.—XII. On the Comet discovered in the Constellation Pegasus in 1821: and on the luminous Appearance observed on the dark Side of the Moon on February 5, 1821. Communicated in a Letter to J. F. W. Herschel, Esq., Foreign Secretary, from Dr. Olbers of Bremen.—XII. On a luminous Appearance seen on the dark Part of the Moon in May 1821. Communicated in a Letter to the Rev. Dr. Pearson, from the Rev. M. Ward.—XIV. On the Occultations of Fixed Stars by the Moon: on the Repeating Circle: on the Perturbations, &c. of the new Planets: and Observations of the late Comet and of the Planet Vesta. Communicated in a Letter to the Rev. T. Catton, F.R.S., from Professor Littrow of Vienna.—XV. On the Places of 145 new Double Stars. By Sir William Herschel, President of this Society.—XVI. Universal Tables for the Reduction of the Fixed Stars. By S. Groombridge, Esq., F.R.S. and S.R.A. Nap.—XVIII. Observation of the

the Solar Eclipse which took Place on September 7, 1820. Communicated in a Letter from M. Piazzi to the Foreign Secretary.

Practical Rules for the Restoration and Preservation of Health, and the best Means for invigorating and prolonging Life, by the late celebrated George Cheyne, M.D. F.R.S.

The Quarterly Journal of Foreign Medicine and Surgery, and of the Sciences connected with them; with Reviews (now added) of British Medical Science and original Cases and Communications. No. XIII. 4*s*. 6*d*.

Elements of Astronomy. By A. Picquot. 12mo. 7*s*. 6*d*.

Botanical Rambles; designed as an easy and familiar Introduction to the elegant and pleasing Study of Botany. By the Author of the Indian Cabinet.

A Monograph on the Genus Camellia. By Samuel Curtis, F.L.S. Illustrated by five Plates, exhibiting eleven Varieties of the Camellia, accurately drawn from Nature by Clara Maria Pope. Large folio. 3*l*. 3*s*. plain; 6*l*. 16*s*. 6*d*. beautifully coloured.

A Description of the Island of St. Michael; with Remarks on the other Azores or Western Islands; originally communicated to the Linnæan Society of England. By John Webster, M.D., &c. 8vo. 13*s*.

Illustrations of the History, Manners, Customs, Arts, Sciences, and Literature of Japan. Selected from Japanese MSS. by M. Titsingh; with coloured Engravings. Royal quarto. 2*l*. 18*s*.

Chart of Van Diemen's Land, from the best Authorities, and from Surveys by G. W. Evans, Surveyor-General of the Colony. 7*s*. 6*d*. coloured, in a case.

History of Cultivated Vegetables. By Henry Phillips. 2 vols. 8vo. 1*l*. 11*s*. 6*d*.

A Letter to Charles Henry Parry, M.D. &c. on the Influence of Artificial Eruptions in certain Diseases incidental to the Human Body. By Edward Jenner, Esq. M.D. &c. Quarto.

The Principles of Medicine, on the Plan of the Baconian Philosophy. Vol. I. on Febrile and Inflammatory Diseases. By R. D. Hamilton. 8vo. 9*s*.

A Treatise on Dyspepsia, or Indigestion: with Observations on Hypochondriasis and Hysteria. By James Woodforde, M.D. 8vo. 5*s*.

Preparing for Publication.

Mr. Farmer has in the press a Second Edition of his popular Work on Head Aches and Indigestion, with considerable additions and improvements.

A new and very improved Edition of the *Pharmacopœia Chirurgica,*

rurgica, under the title of "The Modern Medico-chirurgical Pharmacopœia," containing formulæ for topical and constitutional Remedies, from the private and Hospital Practice of the most eminent Surgeons of London, Edinburgh, Dublin, and the provincial Infirmaries, as well as those of France, Germany, and Italy.

A System of Mechanical Philosophy. By the late John Robison, LL.D., Professor of Natural Philosophy in the University, and Secretary to the Royal Society, of Edinburgh. Edited by David Brewster, LL.D., F.R.S.E.—A copious article on the History and Operations of the Steam Engine has been completely revised by the late James Watt, Esq. and his Son, of Soho.

A System of analytical Geometry. By the Rev. Dionysius Lardner, A.M. of the University of Dublin, and M.R.I.A.

Practical Observations on Paralytic Affections, St. Vitus's Dance, Deformities of the Chest and Limbs. Illustrative of the beneficial Effects of Muscular Action. By W. Ward.

Conversations on Mineralogy. With Plates by Lowry.

Since *Cast-Iron* has been found to be so valuable a material for various parts of buildings and machines, an easy mode of computing its strength has been a desideratum among mechanics and others. A small Work on this subject is now in the press, being a Practical and Experimental Essay on the Strength of Cast-Iron, with Rules, Examples, and Tables. Illustrated by Four Engravings. By Thomas Tredgold, Author of the Article Joinery in the Supplement to the *Encyclopædia Britannica*, and of a Treatise on Carpentry, Timber, and the Dry-rot, &c.

XLIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Jan. 31. A paper by John Goldingham, Esq. F.R.S. was read, containing Observations on the Length of the Seconds Pendulum at Madras.

Feb. 7, 14 and 21. The Meetings on these evenings were occupied in reading a Paper by the Rev. W. Buckland, F.R.S., giving an Account of an assemblage of Fossil Teeth and Bones belonging to extinct Species of Elephant, Rhinoceros, Hippopotamus, and Hyæna, and some other Animals discovered in a Cave at Kirkdale, near Kirby Moorside, Yorkshire.

This paper gives a detailed account of an antediluvian den of hyænas discovered last summer at Kirkdale, near Kirby Moorside, in Yorkshire, about 25 miles north-east of York.

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The den is a natural fissure or cavern in oolitic limestone extending 300 feet into the body of the solid rock, and varying from two to five feet in height and breadth. Its mouth was closed with rubbish, and overgrown with grass and bushes, and was accidentally intersected by the working of a stone quarry. It is on the slope of a hill, about 100 feet above the level of a small river, which, during great part of the year, is engulfed. The bottom of the cavern is nearly horizontal, and is entirely covered to the depth of about a foot, with a sediment of mud deposited by the diluvian waters. The surface of this mud was in some parts entirely covered with a crust of stalagmite; on the greater part of it there was no stalagmite. At the bottom of this mud, the floor of the cave was covered from one end to the other with teeth and fragments of bone of the following animals: hyæna, elephant, rhinoceros, hippopotamus, horse, ox, two or three species of deer, bear, fox, water-rat, and birds.

The bones are for the most part broken, and gnawed to pieces, and the teeth lie loose among the fragments of the bones; a very few teeth remain still fixed in broken fragments of the jaws. The hyæna bones are broken to pieces as much as those of the other animals. No bone or tooth has been rolled, or in the least acted on by water, nor are there any pebbles mixed with them. The bones are not at all mineralized, and retain nearly the whole of their animal gelatin, and owe their high state of preservation to the mud in which they have been imbedded. The teeth of hyænas are most abundant; and of these, the greater part are worn down almost to the stumps, as if by the operation of gnawing bones. Some of the bones have marks of the teeth on them; and portions of the fæcal matter of the hyænas are found also in the den. These have been analysed by Dr. Wollaston, and found to be composed of the same ingredients as the *album græcum*, or white fæces of dogs that are fed on bones, viz. carbonate of lime, phosphate of lime, and triple phosphate of ammonia and magnesia; and, on being shown to the keeper of the beasts at Exeter Change, were immediately recognised by him as the dung of the hyæna. The new and curious fact of the preservation of this substance is explained by its affinity to bone.

The animals found in the cave agree in species with those that occur in the diluvian gravel of England, and of great part of the northern hemisphere; four of them, the hyæna, elephant, rhinoceros, and hippopotamus, belong to species that are now extinct, and to genera that live exclusively in warm climates, and which are found associated together only in the southern portions of Africa near the Cape. It is certain from the evidence afforded by the interior of the den (which is of the same kind with that afforded by the ruins of Herculaneum and Pompeii) that all these animals

animals lived and died in Yorkshire, in the period immediately preceding the deluge; and a similar conclusion may be drawn with respect to England generally, and to those other extensive regions of the northern hemisphere where the diluvian gravel contains the remains of similar species of animals. The extinct fossil hyæna most nearly resembles that species which now inhabits the Cape, whose teeth are adapted beyond those of any other animal to the purpose of cracking bones, and whose habit it is to carry home parts of its prey to devour them in the caves of rocks which it inhabits. This analogy explains the accumulation of the bones in the den at Kirkdale. They were carried in for food by the hyænas; the smaller animals, perhaps, entire; the larger ones piecemeal; for by no other means could the bones of such large animals as the elephant and rhinoceros have arrived at the inmost recesses of so small a hole, unless rolled thither by water; in which case, the angles would have been worn off by attrition, but they are not.

Judging from the proportions of the remains now found in the den, the ordinary food of the hyænas seems to have been oxen, deer, and water-rats; the bones of the larger animals are more rare; and the fact of the bones of the hyænas being broken up equally with the rest, added to the known preference they have for putrid flesh and bones, renders it probable that they devoured the dead carcases of their own species. Some of the bones and teeth appear to have undergone various stages of decay by lying at the bottom of the den while it was inhabited, but little or none since the introduction of the diluvian sediment in which they have been imbedded. The circumstances of the cave and its contents are altogether inconsistent with the hypothesis, of all the various animals of such dissimilar habits having entered it spontaneously, or having fallen in, or been drifted in by water, or with any other than that of their having been dragged in, either entire or piecemeal, by the beasts of prey whose den it was.

Five examples are adduced of bones of the same animals discovered in similar caverns in other parts of this country, viz. at Crawley Rocks near Swansea, in the Mendip Hills at Clifton, at Wirksworth in Derbyshire, and at Oreston near Plymouth. In some of these, there is evidence of the bones having been introduced by beasts of prey; but in that of Hutton Hill, in the Mendips, which contains rolled pebbles, it is probable they were washed in. In the case of open fissures, some may have fallen in.

A comparison is then instituted between these caverns in England, and those in Germany described by Rosenmuller, Esper and Leibnitz, as extending over a tract of 200 leagues, and con-

taining analogous deposits of the bones of two extinct species of bear, and the same extinct species of hyæna that occurs at Kirkdale.

In the German caves, the bones are in nearly the same state of preservation as in the English, and are not in entire skeletons, but dispersed as in a charnel house. They are scattered all over the caves, sometimes loose, sometimes adhering together by stalagmite, and forming beds of many feet in thickness. They are of all parts of the body, and of animals of all ages; but are never rolled. With them is found a quantity of black earth derived from the decay of animal flesh; and also in the newly discovered caverns, we find descriptions of a bed of mud. The latter is probably the same diluvial sediment which we find at Kirkdale. The unbroken condition of the bones, and presence of black animal earth, are consistent with the habit of bears, as being rather addicted to vegetable than animal food, and in this case, not devouring the dead individuals of their own species. In the hyæna's cave, on the other hand, where both flesh and bones were devoured, we have no black earth; but instead of it we find in the *album græcum*, evidence of the fate that has attended the carcases and lost portions of the bones whose fragments still remain.

Three-fourths of the total number of bones in the German caves belong to two extinct species of bear, and two-thirds of the remainder to the extinct hyæna of Kirkdale. There are also bones of an animal of the cat kind (resembling the jaguar or spotted panther of South America) and of the wolf, fox, and polecat, and rarely of elephant and rhinoceros*.

The bears and hyæna of all these caverns, as well as the elephant, rhinoceros, and hippopotamus, belong to the same extinct species that occur also fossil in the diluvian gravel, whence it follows that the period in which they inhabited these regions was that immediately preceding the formation of this gravel by that transient and universal inundation which has left traces of its ravages committed at no very distant period over the surface of the whole globle, and since which, no important or general physical changes appear to have affected it.

Both in the case of the English and German caverns, the bones under consideration are never included in the solid rock; they occur in cavities of limestone rocks of various ages and formations, but have no further connexion with the rocks them-

* M. Rosenmuller shows that the bears not only lived and died, but were also born, in the same caverns in which their bones have been thus accumulated, and the same conclusion follows from the facts observed in the cave in Yorkshire.

selves;

selves, than that arising from the accident of their being lodged in cavities produced in them, by causes wholly unconnected with the animals, that appear for a certain time to have taken possession of them as their habitation.

Feb. 28. Communication of a curious Appearance lately observed upon the Moon, by the Rev. Fearon Fallows, in a Letter addressed to John Barrow, Esq.

On the difference in the Appearance of the Teeth and the Shape of the Skull in different Species of Seals. By Sir Everard Home, Bart.

March 7. Experiments and Observations on the Development of magnetical Properties in Steel and Iron by Percussion. By William Scoresby, Jun. Esq. Communicated by the President.

ASTRONOMICAL SOCIETY OF LONDON.

March 8. A letter was read from M. Gauss, respecting a very simple contrivance for a signal, in geodetical operations, which may be seen at an immense distance. This contrivance is nothing more than the common reflecting speculum of a sextant; being about two inches long, and an inch and a half broad; and mounted in such a manner that it may always reflect the solar rays to the given distant point, notwithstanding the motion of the sun. The instrument, thus mounted, he calls a *heliotrope:* and the reflected light was so powerful that, at 10 miles distant, it was too bright for the telescope of the theodolite, and it was requisite to cover a part of the mirror. At 25 miles distant, the light appeared like a beautiful star, even when one of the stations was *enveloped in fog and rain:* and at 66 miles distant, it was still sufficiently powerful as a signal. In fact, the only limit which appears to the use of this beautiful instrument, is that which arises from the curvature of the earth.

This Society has just announced the publication of the first volume of their MEMOIRS: which must be highly interesting to every lover of Astronomy. With a true zeal for the science, they have resolved to present copies to all their ASSOCIATES, and to most of the scientific Societies and Academies in Europe, Asia, and America: whereby their labours will be more generally known, and duly appreciated.

L. *Intelligence and Miscellaneous Articles.*

EXPLOSION OF A GASOMETER.

On the 15th of March, about four o'clock P. M., one of the gasometers in Friar-street burst with a dreadful explosion. In this

this place is the reservoir of gas for supplying Blackfriars-road and the adjacent streets. The gasometer was quite new, and at the time of the accident contained about 160 tons of water. John Morgan, an engineer, was thrown from the gasometer full ten yards over the wall of Mr. Andrews's premises in Green-street, and killed on the spot. The damage done to the neighbourhood was very considerable, and a great many persons were severely hurt. Mr. Roper's (a bone-boiler) premises were completely destroyed, and he narrowly escaped with his life. Several houses and other manufactories have been much injured. But the most afflicting scene of all, is the calamity suffered by Mrs. Clarke, whose husband was on Wednesday last scalded to death, by losing his hold, and falling into a cauldron of boiling water in Green-street. The power of the water was such on the bursting of the gasometer, that it completely washed away Mrs. Clarke's house, and a little girl in arms was dreadfully hurt, and carried away by the force of the water nearly fifty yards from whence the house stood.

On Monday the 18th of March, a coroner's inquest was held on the body of Morgan, when the following evidence on the subject was given:

Thomas Mees, a smith, stated, that he was sent up to London in the beginning of June, and was employed to put up the tanks: two of them were put up before Christmas, and that which burst since. A month since a crack appeared in one of the plates of the tank, which enlarged, and broke out above, where a new patch was put. Witness and the deceased were employed to repair it, and caused another plate to be made to cover the fissure. On this being done, they found that the first patch must be taken off, to have holes drilled for the screws; a piece of pasteboard and then the new plates were placed over the crack, and supported by a piece of wood, which rested on the adjoining wall, and made every thing water-tight. While they were putting in the screws, the iron hoop, which was the main support of the tank, burst, and dropped off. Witness then said to the deceased, "The tank is sure to burst," and they both ran down some distance from it; but as it held together, they agreed to endeavour to mend the hoop, and returned for that purpose, when the deceased proceeded to climb up the side of the tank to throw down some dust to stop the cracks, as the water was running out fast. Witness again called out, "Come away, it's sure to burst," and stooped down to pick up the fallen hoop, when the tank gave way, and the water carried witness away about eighteen yards. On recovering himself, he found the deceased in a timber-yard, about thirty yards distant, where he had been washed over a shed by the impetuosity of the water; he groaned when witness picked him up, and the blood flowed freely from his ears: he continued

groaning

groaning and bleeding till he was taken to a public-house, where he died in a few minutes. The tank was forty-three feet eight in diameter, eighteen feet deep; it was nearly full of water. Witness could not tell what caused the fissure; he did not think there was any flaw in the iron plates; he never observed the foundation give way. The remaining tanks were fourteen inches only less than the one which had burst. Several of the Jury commented on the dangerous situation of the premises, and said they ought not to be placed so near the habitations of the surrounding neighbourhood.

Mr. Percival (a Juror) said but few of the plates exceeded half an inch in thickness in the centre, and contended they were not sufficiently strong; he spoke from his own knowledge, for he knew the plates for vats were generally an inch and an inch and a half in thickness.—Another Juror said, that if it was only made of tin, and the hoops were sufficiently strong, it would not break. Mr. Percival resumed, and said, the hoop was not stronger than that which he had round a vat that contained only two tons of water.—Robert Monro, Esq. a Director of the Gas Company, said the tank contained 752 tons of water. The plates were three-quarters of an inch in thickness; some were stouter. The works were furnished by contract; the ironmasters engaging to make them water-tight. The contractors had put up eleven tanks in London, which were all standing. The whole loss (the tank cost between 700*l.* and 800*l.*) would fall on the contractors. It was one foot longer than the others, but he did not know whether the iron was made proportionally stronger. Of course the building was left to the contractors, and it was their interest to make the tanks of sufficient strength.

SUCCESSFUL METHOD FOLLOWED IN THE UKRAINE FOR THE CURE OF HYDROPHOBIA*.

When Mr. Marochetti, an operator in the Hospital at Moscow, was in the Ukraine in 1813, in one day fifteen persons applied to him for cure, having been bitten by a mad dog. Whilst he was preparing the remedies, a deputation of several old men made its appearance to request him to allow a peasant to treat them; a man who for some years past enjoyed a great reputation for his cures of hydrophobia, and of whose success Mr. Marochetti had heard much. He consented to their request under these conditions: 1st, that he, Mr. Marochetti, should be present at every thing done by the peasant; 2dly, in order that he might be more fully convinced that the dog was really mad, he (Mr. M.) should select one of the patients, who should be treated according

* From the Berlin State Gazette of the 14th February 1822.

ing to the medical course usually held in estimation. A girl of six years old was chosen for this purpose. The peasant gave to his fourteen patients a strong "decoction" of the "*Summit,*" and "*Fl. Genista luteæ tinctoriæ,*" (about 1½lb. daily,) and examined twice a day under the tongues, where, as he stated, small knots, containing the poison of the madness, must form themselves. As soon as these small knots actually appeared, and which Mr. Marochetti himself saw, they were opened, and cauterised with a red-hot needle; after which the patient gargled with the decoction of the "*Genista.*"—The result of this treatment was, that all the fourteen (of whom only two, the last bitten, did not show these knots) were dismissed cured at the end of six weeks, during which time they drank this decoction. But the little girl, who had been treated according to the usual methods, was seized with hydrophobic symptoms on the seventh day, and was dead in eight hours after they first took place. The persons dismissed as cured were seen three years afterwards by Mr. Marochetti, and they were all sound and well. Five years after this circumstance (in 1818) Mr. Marochetti had a new opportunity in Podolia of confirming this important discovery. The treatment of twenty-six persons, who had there been bitten by a mad dog, was confided to him; nine were men, eleven women, and six children. He gave them at once a decoction of the "*Genista,*" and a diligent examination of their tongues gave the following result: Five men, all the women, and three children, had the small knots already mentioned; those bitten worst, on the third day, others on the fifth, seventh, and ninth; and one woman, who had been bitten but very superficially in the leg only, on the twenty-first day. The other seven also, who showed no small knots, drank the "*decoctum Genistæ*" six weeks, and all the patients were cured.

In consequence of these observations, Mr. Marochetti believes that the hydrophobic poison, after remaining a short time in the wound, fixes itself for a certain time under the tongue, at the openings of the ducts of the "glandular sub-maxiller," which are at each side of the tongue-string, and there forms those small knots, in which one may feel with a probe a fluctuating fluid, which is that hydrophobic poison. The usual time of their appearance seems to be between the third and ninth day after the bite; and if they are not opened within the first twenty-four hours after their formation, the poison is re-absorbed into the body, and the patient is lost beyond the power of cure. For this reason, Mr. Marochetti recommends that such patients should be immediately examined under the tongue, which should be continued for six weeks, during which time they should take daily 1½lb. of the "*decoct. Genist.*" (or four times a day the powder, one drachm *pro dosi*). If the knots do not appear in

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the day-time, no madness is to be apprehended; but, as soon as they show themselves, they should be opened with a lancet, and then canterised, and the patient should gargle assiduously with the above-mentioned "*decoct.*"

We hasten to convey to our readers this important discovery, (which we borrow from the Petersburgh Miscellaneous Treatises in the Realm of Medical Science for 1821), which certainly deserves the full attention of all medical practitioners; and which, if confirmed by experience, may have the most beneficial results.

STATUE OF ISIS IN THE BRITISH MUSEUM*.

Among the beautiful specimens of Egyptian sculpture, which at once annihilates every argument of Winkelman's, and other learned antiquaries, who would condemn its principles as meagre, hard, and unfaithful to nature, may be cited the most exquisite fragment of a female statue, probably of Isis, now lying in the vestibule of the British Museum. This figure is perfect from the waist, and measures about five feet. It is formed of one block of white marble, and is executed with a softness and symmetrical beauty that vie with any statues of antiquity.

The face appears to be the goddess Isis, and while it presents the Nubian cast of features, it is so delicately formed, that it breathes a most peculiar and winning softness of expression. The cheeks are high and prominent, but finely rounded and full; the eyes so sharply sculptured, that they seem finished but yesterday. The mouth is all but breathing; the lips having the marked breadth of expression, so perfectly the Egyptian style, with the small but highly important edge that marks their curve in speaking, which might appear on the eve of taking place, from the masterly delineation of the mouth. This fine head was crowned by an asp diadem, with the usual folds or lappets falling down on the chest, as appears in all the figures of Isis, with the Nubian features represented on the sycamore sarcophagi which inclose the mummies. She has also the collar (the Rabid of the initiation), which is most delicately sculptured. Indeed, the impressions which the contemplation of this figure excite, are those of wonder and astonishment, that a form of such beauty could have been the workmanship of an Egyptian artist. It has excellencies that will not fade by a comparison with any Grecian or Roman form that adorns the Museum, and the Egyptian goddess possesses the charm of attracting and riveting the imagination, and filling up a *beau idéal* of character equally with any of the *chef-d'œuvres* of the collection, and which arises from the extraordinary individuality which its expressive contour, and inviting smile, pecu-

* Gentleman's Magazine, Jan. 1822.

liarly associate with it; as is also the case with the celebrated Memnon's head, and all the higher class of Egyptian sculpture. Those, therefore, who contemplate these features and form, will acquire far higher notions of the excellence of Egyptian art than hitherto has been ascribed to it.

The classic writers of Greece and of Rome have always declared Egypt to be the fountain and source of knowledge. These countries have borrowed their rules of art, and transported their obelisks to adorn their colonnades and forums; and Rome and the whole world, unto our own æra, have done full justice to the vast conceptions, the colossal and gigantic proportions of their temples, their statues and their obelisks, and above all, to the indestructible material they selected with such boldness and hardihood for their extraordinary labours, which defies all competition of modern skill, being of the basalts and oriental granite, hard and impenetrable to the edge of all modern tools. To these genuine principles of grandeur and sublimity, developed in their vastness and eternal duration, this pleasing and delicately formed statue, as well as many of the busts and precious relicks collected for the last ten years from this ancient land, now lay claim also to the majestic and the beautiful. They differ indeed in many striking essentials from the celebrated statues of Greece and of Rome, but they combine in themselves such excellencies, as to render a disquisition into their first principles of composition very desirable; and placed as they now are in the vestibule even of the Elgin marbles, the works of Phidias, in the face almost of those forms of matchless excellence, it would be highly pleasing to trace how, in such a fearful collision, they still maintain their attraction, and by what charm they thus fascinate their beholder to linger around their austere and smiling forms, which appear breathing forth through lips all but animated, the astonishing and mystic secrets of their venerable forms.

CANAL BOATS.

The following account of Mr. T. M. Van Heythuysen's patent for propelling barges or boats through canals, has been sent us by a correspondent.

"The object of this invention is to substitute manual labour instead of equestrian in transporting barges through canals, and is simply thus: A tread-wheel is fixed either to the fore-, or both to the fore- and after-part of a barge, which is trod round. The axle passes through the tread-wheel and projects from the sides of the barge about 20 inches: to this is fixed a paddle-wheel similar to those used by vessels propelled by steam; each of these wheels contains six paddles. Supposing the man who treads to weigh 135 lbs. and deduct 35 lbs. for friction, he will then

then tread the axle round at a force of 100 lbs. The superiority over the common method is this:—A man when he pulls sculls or oars, pulls them through the water twenty-four times in a minute, and the strength of his pulling is computed at about 30 lbs. each time. By Mr. Van H.'s method the paddles pass through the water 136 times in a minute; and as only two paddles are in the water at the same time, each paddle is passed through the water by a force of 50 lbs. There is not sufficient space on a canal to allow of the use of oars. This newly invented machinery is very simple and can be taken off the vessel in a moment, and so light that a man can walk away with it with as much ease as he can with a pair of oars. Two men can propell a canal barge with this contrivance at the rate of five miles an hour. The expense of keeping track roads for horses to draw the barges, and the expense of keeping the horses themselves, seem to make this a great desideratum to all canal property."

☞ We suspect that the patentee will meet with objections not easy to be overcome respecting the application of such machinery to canal navigation. Even in the present method of moving the barges, when the horses go beyond a certain rate, the motion given to the water tends to wash down the banks;—but what is this compared to the moving tide that would be produced by the working of paddles?

IDENTITY OF CALC-SINTER AND CALCAREOUS SPAR.

The Rev. Dr. Fleming, of Flisk, transmitted to me lately two specimens of this substance, with the following remark: "Lamellar calc-sinter from Macalister's Cave in Sky. I procured these crystals in shallow pools in the cave filled with the calcareous water. The indications of crystallization are distinct, but the crystals seem to be but in progress. The summits of the crystals of the smallest piece are smooth and flat, and indicate the prisms below to be five-sided, and sometimes four-sided. I regard these specimens as exceedingly curious, as they are genuine examples of Neptunian calcareous spar. 2. Acicularly crystallized fibrous Calc-sinter.— This substance is from the Isle of Man; the specimen from which these fragments were separated was given me by Mr. Stevenson several years ago, and is interesting as being a recent aqueous formation." Dr. Fleming adds, "that all the calcareous matter in Macalister's Cave, whatever be its external form, stalactitic, stalagmitic, or encrusting, is all more or less in the state of calcareous spar, with the usually foliated structure: that which lies in the pools or hollows of the caves has its crystalline forms like those in the specimens sent." Upon examining these interesting specimens, I succeeded in extracting from them regular rhombs of calcareous spar, having

their angles of the same value as the finest specimens of carbonate of lime. Their double refraction and their polarising force, were of the same character and the same intensity as the purest Iceland spar. D. B.—(*Edin. Phil. Jour.*)

NEW METAL.

Counsellor Giesse of Dorpat has communicated to the world the discovery of what he at present thinks to be a new metal, extracted from the residue of English sulphuric acid, on distilling it to dryness. One variety left, out of 16 ounces, 9½ grains of a white residuum, in which there was no sulphate of lead. It changed colour several times during the experiments made upon it, and he thinks it was formed of the sulphur employed in manufacturing the acid. It is susceptible of oxidation, and its alkaline combinations form double salts with acids. Still the professor's details are judged, on the whole, to be inconclusive.

SMUT IN WHEAT.

"Take a double handful of good clean wheat, wash it well in clear water in a hand-bason or other utensil, rub the seed well between the hands *in the water*, and change the water several times until it comes from the seed quite clear : then sow half of the washed seed in a corner of the farm garden, or on some other convenient spot, but be careful not to use a rake for covering the seed, that had been recently used in the barn or elsewhere amongst smutted wheat, or even amongst the straw of that wheat. The first part of the wheat being disposed of, procure some smut balls, having no kernels of wheat amongst them ; break the balls in a sample-bag, and put the other half of the washed wheat into the same bag ; shake the wheat and the smut powder well together, and allow the wheat to remain in the bag one or two days, when it will have become dry, and the smut powder have effected the inoculation ; then sow that seed upon a spot of ground contiguous, but not immediately adjoining to where the former handful of seed had been sown. The reason for not depositing one parcel of seed immediately adjoining to the other is, to guard against the probability of the two parcels of seed becoming intermixed, through the agency of birds, mice, &c. as an accident of that nature would render the experiment incomplete ; whereas, if it is properly conducted, the result will assuredly be satisfactory : so much so, that the produce of the first sample will be without smut, and that of the second will be smutted, more or less (probably half smut balls) according to the state of the smut powder at the time the inoculation was effected. Smut balls taken from old wheat are not so liable to communicate the disease, as those taken from new wheat : this phænomenon is owing to the eggs of

of the smut insect becoming addled, or rendered effete, when kept beyond the season assigned by nature for their procreation or reproduction: hence old wheat seed is less liable to produce smut than new wheat; but this depends in some measure upon the manner in which the old wheat had been kept; if in stacks, the insects' eggs will not have been entirely destroyed, because of the air having been excluded from those situated in the middle of the stack; but in the event of the wheat being thrashed out a considerable time previously, the eggs will have become addled, from exposure to the air. The same position holds good in regard to the eggs of other insects, reptiles, or birds: one law of Nature rules the whole; and it even extends to the germ of vegetables, for we see that old wheat seed kept in stacks vegetates better than when kept in granaries. This explanation will sufficiently account for the contrariety of opinion respecting the eligibility of using old wheat for seed, whether for producing a full crop of wheat, or as a prevention of smut."—*Baker's Treatise.*

THE GOLDEN PIPPIN.

Mr. Phillips of Bayswater, who has lately written an historical account of Fruits, has furnished us with some further account of that elegant and excellent little English apple the Golden Pippin, and which we hope will so satisfactorily prove the error of this variety's being lost through sympathy with the parent tree, that it may induce the planters of orchards to return to a cultivation of this favourite apple that produces a cider, which Mr. Phillips tells us surpasses in richness of flavour even "the gay Champaigne."

Mr. Phillips seems not to have confined his inquiries to this country alone as to the correctness of the theory, which had so far gained credit as nearly to banish this favourite apple from our gardens. He tells us that there are at this time a considerable number of the true golden pippin trees growing on the mountains in Madeira, about 14 miles from the capital of that island, and at an elevation of about 3000 feet above the sea, which regularly produce abundance of fruit, notwithstanding the trunks and branches are covered with a white lichen or moss. Grafts which were sent from these trees by Thomas Harrison, Esq. about three years ago, produced fruit at Cheshunt in Hertfordshire the second year, and proved to be the original golden pippin.

In several parts of America these trees are in a thriving state, which has been proved by the excellent quality of the fruit lately sent to this country. In addition to which he tells us he saw, notwithstanding the late unfavourable season, many trees of this variety in Sussex, as healthy in appearance as most other kinds of apples, particularly in the garden of Messrs. Humphreys, at Chichester.

Chichester, where the fruit was of a size and perfection that he had never seen surpassed.

Mr. Phillips admits that the golden pippin is a more delicate tree than many other varieties, but by no means so much so as is generally supposed, and it only requires, as it deserves, the most genial situation of the orchard to render it as prolific as formerly. About the year 1685 Lord Clarendon had, at his seat at Swallowfield, Berks, an orchard of 1000 golden and other cider pippins.

Pippins are said to take their names from the small spots or pips that usually appear on the sides of these kinds of apples, and which is no indication of decay.

BOTANY.

On Christmas-day the following plants, selected from many others, were in flower in the open ground at the Botanic Garden of Oxford, viz.:—1. Polycarpon tetraphyllum.—2. Scabiosa atropurpurea.—3. Cerinthe minor.—4. Symphytum Orientale.—5. Borago officinalis.—6. Echium violaceum.—7. Primula vulgaris.—8. Primula Auricula.—9. Campanula patula.—10. Campanula Rapunculoides.—11. Lonicera implexa.—12. Solanum tomentosum.—13. Solanum nigrum.—14. Vinca major.—15. Sanseviera sessiliflora.—16. Hydrangea hortensis.—17. Dianthus Deltoides.—18. Dianthus Carthusianorum.—19. Reseda odorata.—20. Reseda alba.—21. Papaver Cambricum.—22. Delphinium Consolida.—23. Anemone Hepatica.—24. Anemone coronaria.—25. Alyssum maritimum.—26. Mathiola incana.—27. Erodium moschatum.—28. Erodium Hymenodes.—29. Pelargonium Grossularioides.—30. Fumaria lutea.—31. Fumaria spicata.—32. Arnopogon Dalechampii.—33. Cnicus Eristhales.—34. Gnaphalium fœtidum.—35. Elichrysum bracteatum.—36. Erigeron acre.—37. Tussilago fragrans.—38. Senecio elegans.—39. Mercurialis annua.—40. Parietaria officinalis.

THE BOA CONSTRICTOR SEEN IN THE ISLAND OF ST. VINCENT.

A most singular circumstance occurred last week in the Charaib country, when some negroes, who were working near Sandy Bay, discovered an immense serpent, hitherto wholly unknown in any of these islands, and which was shot through the head by one of the party. It is supposed to be a species of *Boa* so common on the neighbouring continent, but in what way it reached the shores of St. Vincent is quite unknown. Its entire length was between fourteen and fifteen feet, the circumference of the body between three and four feet. When first seen it was lying in a coil, but raised itself on being roused.—*Royal Gazette and Bahama Advertizer*, August 1821.

EARTHQUAKES.

EARTHQUAKES.

Upwards of one hundred acres of the land of Letterbrocken, part of the property of the Provost of Trinity College in Joyce County, and consisting of prime pasture and mountain, on which a number of tenants resided, commenced moving and carrying with it huge rocks, immense masses of earth, the entire crop of wheat, oats, potatoes, &c., precipitated the whole into the sea and disappeared. Previous to its movement, a great noise was heard for some time, and the ground was convulsed. It is supposed that the previous drought which had occurred, prepared the way for this phenomenon. Two days after, a large tract of land thickly inhabited, the property of R. Martin, Esq. M.P., in the same neighbourhood, was visited by a like phenomenon, but even of a more destructive nature; the loss of the sufferers not being confined to their land and crops, but their entire stock and property being swallowed up by the earthquake. These occurrences are mentioned in the *Gent. Mag.* for November, from the *Tuam Gazette*, and their date given as ten days previous.

The Batavian Journals of April give an account of an earthquake very destructive in its effects which took place on the 29th of December 1820, on the south coast of Celebes. It did immense damage, particularly at Boelækomba, where the sea rose several times a prodigious height, and then falling again with great rapidity, alternately deluged and left the shores, destroying all the plantations from Bontain to Boelækomba. Many hundred persons lost their lives. The forts of Boelækomba and Bontain were much damaged.

On the 4th of January this year, another shock of an earthquake occurred in the same neighbourhood.

On the 17th February, at half-past five in the afternoon, several smart shocks of an earthquake were felt at Comorn, in Hungary. The first, which lasted full three seconds, was so severe, that the church of St. Andrew was cracked in several places, and many chimneys of the barracks were thrown down. But the effects of this awful phænomenon were much more sensibly felt at the village of Izso, about two leagues from Comorn, where not only the Catholic and the Protestant church were greatly damaged, but six houses wholly thrown down, and a quantity of cattle buried under their ruins.

Some slight shocks of an earthquake were experienced at Presburg on the 18th of February, at five in the afternoon.

On the 19th of February, an earthquake occurred which was felt at places very distant from each other. It was felt at Paris, at Lyons, and still more violently in Switzerland. At Bourg, three distinct but immediate shocks were felt. The first was attended with a loud detonation: the third was longer and more

smart,

smart. In the eastern communes, at the entrance of the mountains which branch out of the Jura, the shocks were still more violent, and were accompanied with detonations like discharges of artillery. Many houses were damaged.

The Journal of Savoy presents the following particulars respecting this earthquake:—"At Aix they experienced two succeeding shocks, which lasted about seven seconds. The noise was like that we heard here. A number of chimneys fell. The waters, impregnated with sulphur, were of a whitish grey colour, and they continued in a state of agitation near two hours. Their temperature did not vary. All the phænomena were the same as those observed at the earthquake which happened at Lisbon in 1755. At Yenne, where a religious ceremony had called many persons to church, at the moment the preacher had uttered his exordium, 'We are suspended between heaven and hell,' a frightful noise was heard. The vaulted roof of the church opened, and a shower of stones and mortar descended on all sides. It is impossible to describe the scene of desolation which struck the terrified congregation. Their agitation in the dust, and the dreadful screams uttered in their rush to get to the doors, were awful in the extreme; several were trampled under feet, whilst others got into holes and corners to escape death. Many persons are suffering under the effect of this event, but only two persons have received serious wounds,' a circumstance almost incredible. It is a remarkable circumstance, that the earthquake was felt in three other churches, at the very moment when the preachers were pronouncing the words uttered by the preacher at Yenne. At La Motte Servolex, the Curate announced to his parishioners, that if they did not make haste to do penance, immediate punishment would follow their sins. At the same instant the earthquake was felt, and all the congregation fell upon their knees to implore forgiveness of their sins. At the College of Chambery, in one of the lectures upon Death, it was urged that death might strike any one of the pupils in a month, in a day, perhaps that instant. At these words the church shook, and the roof seemed falling on the students, who ran precipitately to the door, uttering a cry of terror."

A letter from Chambery, speaking of the earthquake of 19th February, says,—"The roof of the church of Rumilly opened in several parts, and separated from the lateral walls. The belfry was rent to the extent of one hundred feet; all the springs were disturbed. There were three shocks. One quarter of the town seemed from the neighbouring heights to disappear for a moment behind the other, and the trees seemed to cross each other. During the shock many persons experienced in different parts of the body the same effects that are produced by a strong electric shock."

A violent

A violent shock of an earthquake was also felt at Belley (Ain) on the 23d of February at 35 minutes after 3 o'clock P. M.

EARTHQUAKES AND MAGNETISM.

M. Arago has transmitted to the French Academy of Sciences, an account of an observation he had made which proves that the recent earthquake, the shocks of which were felt at Lyons and its neighbourhood, also extended its action to Paris. M. Arago has an observatory in Paris for the purpose of observing the variations of the magnetic needle. On the 19th of February the needle remained perfectly steady until half past eight o'clock; at a quarter before nine it became agitated in a very extraordinary manner with an oscillatory motion strongly inclining towards the magnetic meridian. On observing this truly singular phænomenon, M. Arago was of opinion that it was occasioned by an earthquake.

At the same day and hour M. Biot remarked an oscillatory movement produced by the same earthquake, at his own residence in the *College de France.*

METHODS OF KINDLING FIRE IN THE SANDWICH ISLANDS.

There are various methods of producing fire. In the Caroline Islands, a piece of wood being held fast on the ground, another short piece, about a foot and a half long, of the thickness of a thumb, even as if turned, and with the end bluntly rounded off, is held perpendicularly over it, and put in motion between the palm of the hand, like the mill used for making chocolate. The motion is at first slow, but is accumulated, and the pressure increased, when the dust produced by the friction collects round the bores, and begins to be ignited. This dust is the tinder which takes fire. The women of Eap are said to be uncommonly clever at this process. In Radack and the Sandwich Islands, they hold on the under piece of wood another piece a span long, with a blunt point, at an angle of about 30 degrees, the point of the angle being turned from the person employed. They hold the piece of wood with both hands, the thumbs below, the fingers above, so that it may press firmly and equally, and thus move it backwards and forwards in a straight line, about two or three inches long. When the dust that collects in the groove, produced by the point of the stick, begins to be heated, the pressure and the rapidity of the motion are increased. It is to be observed, that in both methods two pieces of the same kind of wood are used; for which purpose, some of equally fine grains, not too hard, and not too soft, are the best. Both methods require practice, dexterity, and patience. The process of the Aleutians is the first of these methods, improved by mechanism. They ma-

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nage the upright stick in the same manner as the gimlet or borer which they employ in their work. They hold and draw the string, which is twice wound round it with both hands, the upper end turning in a piece of wood, which they hold with their mouth. In this way, I have seen a piece of fir turned on another piece of fir, produce fire in a few seconds; whereas, in general, a much longer time is required. The Aleutians also make fire by taking two stones with sulphur rubbed on them, which they strike together over dry moss strewed with sulphur.—(*Kotzebue's Voyage*, 3. 259.)

AMERICAN ASYLUM FOR THE DEAF AND DUMB.

An examination of the pupils of the New York Institution, for the instruction of the deaf and dumb, took place at that city, on the 25th of October 1821. The number of unfortunates were sixty, who excited much interest by the manner in which they went through their exercises. A Miss Barnard from Utica expressed in signs the Lord's prayer, and no one could fail to understand her. Her attitude was devotional, her gestures graceful and significant, her countenance expressive, and her whole performance indicated a knowledge of what her signs expressed: she had only been under instruction fourteen months.

The exercise which followed was one of memory, and in this several took part. Among the rest Miss Barnard reduced to writing the Lord's prayer, which she had previously rendered by signs. Another pupil wrote the history of the creation—a third, the flood—a fourth, the ten commandments—while another wrote from memory the character of Christ—and a sixth, the miracle of Christ curing the deaf and the blind.

Next followed two small girls, not more than nine or ten years old, who conjugated, by writing on the black board, two verbs through several of the tenses, in connexion with the personal pronouns, and a noun, forming a complete sentence; as, *I curl my hair—I curled my hair—I wash my hands*, &c. This was explained by Mr. Loofborrow, the principal teacher, as the method practised in the New York school for the deaf and dumb, and as involving a principle not adopted in common schools, and which might be beneficially introduced. Children generally learn grammar by rote; but as the object of grammar is to teach them the structure of language, it would be better, in going through the moods and tenses of the verbs, to prefix the pronouns and add a noun as in the instances above.

The exercise which followed was the fable of the Bear and the Bees, from Æsop, told in signs by Richard Sip, the son of an able farmer in New Jersey. This went to show that the deaf and dumb understand the nature of a fable and its application.

MEASURE-

MEASUREMENT OF THE MERIDIAN IN RUSSIA.

A series of operations for a new measure of the meridian in the Russian provinces of the Baltic, will take place during the summer. M. Struve, professor of Astronomy, and rector of the university of Dorpat, will commence his labours at the 56th degree of north latitude, on the meridian of the observatory of the university of Dorpat. The expenses will be defrayed by the university. The emperor has given 2000 ducats to procure the necessary instruments, and Dr. Walbeck of the Swedish university of Abo will act in concert with professor Struve to render the measure more complete.

AEROLITES.

A large aërolite fell on the 15th June last at Juvinas, a village in the arrondissement of l'Argentière, department de l'Ardèche, respecting which some very accurate details have been preserved. It fell about four o'clock P.M., the sky being clear, and the sun shining bright; a continued rolling noise was heard for above three minutes, during which time four distinct detonations took place. The noise was heard at Tarascon, at Nismes, and still further off. A brilliant fire was seen in the air by persons at Nismes, St. Thome, (a league to the west of Viviers,) and Aps, a league further off. All agree in saying it resembled a fire burning like a star, and descending slowly in the N.W., and which on disappearing left a train of smoke. Search was made in the ground where the fire descended; and at the depth of five feet a large stone was discovered weighing 220lbs., or 91 kilogrammes.

In a further account of this aërolite given by M. L. A. D. Firman, it is stated that another meteoric stone a kilogramme in weight was found a little distance off, and several small ones at Mayras near Juvinas. M. de Malbos, who happened to be at Barias when the stone fell, was looking towards the place at the time when it first appeared. He saw a globe of fire descend perpendicularly from the heavens. He showed it to his workmen, and counting his pulse estimated the time between its appearance and the explosion that followed, at five seconds. He observed also the obscure vapoury trace left by the meteorolite in the air. It was not continued to the earth, but ceased to be emitted before the stone reached the ground, and remained seven or eight minutes undissipated.—*Journ. de Phys.* xciii. page 71.

EXTRAORDINARY HAIL STORM.

On the 27th of June last, at Usnaw in the government of Riew, in Russia, there fell a shower of hailstones so large and hard that they killed a flock of 200 sheep, and cruelly mutilated the shepherd and his dogs.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Erskine Cochrane, esq. of Somerset-street, Portman-square, for certain improvements in the construction of lamps, whereby they are rendered capable of burning concrete oils, animal fat, and other similar inflammable substances.—Dated 23d February 1822.—6 months allowed to enroll specification.

To John William Buckle, of Mark-lane, London, merchant, who in consequence of a communication made to him by John Parker Boyd, of Boston in the United States, is in possession of certain improvements in machinery for shaping or cutting out irregular forms in wood or any other materials or substances, which admit of being cut by cutters or tools revolving with a circular motion, whether such motion be continuous or reciprocating.—2d March.—2 months.

To John Higgins, of Fulham, Middlesex, esq. for certain improvements upon the construction of carriages, which he conceives will be of great public utility.—2d March.—6 months.

To Charles Yardley, of Camberwell, glue-manufacturer, for a method of manufacturing glue from bones by means of steam, which invention he believes will be of general benefit and advantage.—2d March.—2 months.

To John Thompson, of Regent-street, Westminster, for certain improvements in the method of forming or preparing steel for the manufacture of springs for carriages, but principally applicable to all those usually denominated coach springs.—2d March.—2 months.

To John Ruthven, of Edinburgh, printer, for a method of procuring a mechanical power.—2d March.—4 months.

To George Stratton, of Hampstead Road, Middlesex, engineer, for an improved process of consuming smoke.—2d March.—6 months.

To James Gladstone, of Liverpool, Lancashire, iron-monger, for a chain of an improved construction, which he conceives will be of great public utility.—12th March.—6 months.

To Bartlett Bate, of the Poultry, London, optician, for certain improvements upon hydrometers and saccharometers, which invention he believes will be of much benefit and utility.—21st March.—2 months.

To William Eugene Edward Conwell, of Madras, but now residing at Ratcliff Highway, Middlesex, surgeon, for his discovered improvement in the preparation and application of a certain purgative oil.—21st March.—6 months.

To Samuel Robinson, of Leeds, cloth-dresser, for certain improvements on a machine for shearing and cropping woollen cloth.—21st March.—6 months.

POPULATION.

Comparative Statement of the Population of the several Counties of Great Britain and Ireland; the former for the Years 1801, 1811, and 1821; and the latter for 1813 and 1821.

ENGLAND.

COUNTIES.	1801.	1811.	1821.
Bedford	63,393	70,213	83,716
Berks	109,215	118,277	131,977
Buckingham	107,444	117,650	134,068
Cambridge	89,346	101,109	121,909
Chester	191,751	227,031	270,098
Cornwall	188,269	216,667	257,447
Cumberland	117,230	133,744	156,124
Derby	161,142	185,487	213,333
Devon	343,001	383,308	439,040
Dorset	115,319	124,693	144,499
Durham	160,361	177,625	207,673
Essex	226,437	252,473	289,424
Gloucester	250,809	285,514	335,843
Hereford	89,191	94,073	103,231
Hertford	97,577	111,654	129,714
Huntingdon	37,568	42,208	48,771
Kent	307,624	373,095	426,016
Lancaster	672,731	828,309	1,052,859
Leicester	130,081	150,419	174,571
Lincoln	208,557	237,891	283,058
Middlesex	818,129	953,276	1,144,531
Monmouth	45,582	62,127	71,833
Norfolk	273,371	291,999	344,368
Northampton	131,757	141,353	162,483
Northumberland	157,101	172,161	198,965
Nottingham	140,350	162,900	186,873
Oxford	109,620	119,191	134,327
Rutland	16,356	16,380	18,487
Salop	167,639	194,298	206,266
Somerset	273,750	303,180	355,314
Southampton	219,656	245,080	282,203
Stafford	239,153	295,153	341,824
Suffolk	210,431	234,211	270,542
Surrey	269,043	323,851	398,658
Sussex	159,311	190,083	232,927
Warwick	208,190	228,735	274,392
Westmoreland	41,617	45,922	51,359
Wilts	185,107	193,828	222,157
Worcester	139,333	160,546	184,424
York, E. Riding	139,433	167,353	190,709
—— N. Riding	155,506	152,445	183,694
—— W. Riding	563,953	653,315	800,848
Totals	8,331,434	9,538,827	11,260,555

WALES.

COUNTIES.	1801.	1811.	1821.
Anglesea	33,806	37,045	45,063
Brecon	31,633	37,735	43,613
Cardigan	42,956	50,260	57,311
Carmarthen	67,317	77,217	90,239
Carnarvon	41,521	49,336	57,958
Denbigh	60,352	64,240	76,511
Flint	39,622	46,518	53,784
Glamorgan	71,525	85,067	101,737
Merioneth	29,506	30,924	33,911
Montgomery	47,978	51,931	59,899
Pembroke	56,280	60,615	74,009
Radnor	19,050	20,900	23,073
Totals	541,546	611,788	717,108

SCOTLAND.

COUNTIES.	1801.	1811.	1821.
Aberdeen	123,082	135,075	155,141
Argyll	71,859	85,585	96,165
Ayr	84,306	103,954	127,299
Banff	35,807	36,668	43,561
Berwick	30,621	30,779	33,388
Bute	11,791	12,033	13,797
Caithness	22,809	23,419	30,238
Clackmannan	10,858	12,010	13,263
Dumbarton	20,710	24,189	27,317
Dumfries	54,597	62,960	70,878
Edinburgh	122,954	148,607	191,514
Elgin	26,705	28,108	31,162
Fife	93,743	101,272	114,556
Forfar	99,127	107,264	113,430
Haddington	29,986	31,164	35,127
Inverness	74,292	78,336	90,157
Kincardine	26,349	27,439	29,118
Kinross	6,725	7,245	7,762
Kirkcudbright	29,211	33,684	38,903
Lanark	146,699	191,752	244,387
Linlithgow	17,844	19,451	22,685
Nairn	8,257	8,251	9,006
Orkney & Shetl.	46,824	46,153	53,124
Peebles	8,735	9,935	10,046
Perth	126,366	135,093	139,050
Renfrew	78,056	92,596	112,175
Ross & Cromarty	55,343	60,853	68,828
Roxburgh	33,682	37,230	40,892
Selkirk	5,070	5,889	6,637
Stirling	50,825	58,174	65,331
Sutherland	23,117	23,629	23,840
Wigtown	22,918	26,891	33,240
Totals	1,599,068	1,805,688	2,092,014

IRELAND.

:	1813.	1821.	MUNSTER:	1813.	1821.
Carlow............	69,566	81,287	Clare	160,603	209,595
Drogheda, Town	16,123	18,118	Cork	523,936	702,000
Dublin...........	110,437	160,274	Cork, City....	64,394	100,535
Dublin, City....	176,610	186,276	Kerry	178,622	205,037
Kildare	85,133	101,715	Limerick	103,865	214,286
Kilkenny.........	134,664	157,096	Limerick, City .	no return.	66,042
Kilkenny, City	no return.	23,230	Tipperary	290,531	353,402
King's County	113,226	132,319	Waterford	119,457	127,679
Longford.........	95,917	107,702	Waterford, City	25,467	26,787
Louth	no return.	101,070			
Meath	142,479	174,716		—	2,005,363
Queen's County	113,857	129,391			
Westmeath	no return.	128,042			
Wexford	no return.	169,304			
Wicklow	83,109	115,162			
	—	1,785,702			

:			CONNAUGHT:		
Antrim	231,548	261,601	Galway	140,995	286,921
Armagh	121,449	196,577	Galway Town .	24,684	27,827
Carrickfergus T.	6,136	8,255	Leitrim	94,095	105,976
Cavan	no return.	194,330	Mayo	237,371	297,538
Donegal	no return.	249,483	Roscommon...	158,110	207,777
Down	287,290	329,348	Sligo	no return.	127,879
Fermanagh ...	111,250	130,399			
Londonderry . .	186,181	194,099		—	1,053,918
Monaghan ...	140,433	178,183			
Tyrone	250,746	259,691			
	—	2,001,966			

Summary of 1821.

Leinster	1,785,702
Munster	2,005,363
Ulster	2,001,966
Connaught	1,053,918
Total in Ireland ...	6,846,949

Summary of Great Britain and Ireland.

	1801.	1811.	1821.
England	8,331,434	9,538,827	11,260,555
Wales	541,546	611,788	717,108
Scotland	1,599,068	1,805,688	2,092,014
	10,472,048	11,956,303	14,069,677
Army and Navy .	470,598	640,500	310,000
Total of Gt. Britain	10,942,646	12,596,803	14,379,677
Ireland			6,846,949
Grand total of population of Great Britain and Ireland in 1821			21,226,626

The population of the Islands in the British Seas not having been ascertained in 1801 and 1811, no comparative statement of them can be given; but the existing population of those Islands, when enumerated in the year 1821, appears to have been as follows:

Isle of Man	20,827
Guernsey and its dependent Islets	40,084
Jersey	28,600
Scilly Isles	2,614
	92,125

MRS. IBBETSON'S PAPERS.

Our readers are requested to observe that the note attached to the bottom of p. 5, in our January Number, should have been inserted at the end of the first paragraph of Mrs. Ibbetson's paper in our February Number, p. 82.

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE,

BY MR. SAMUEL VEALL.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1822.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Feb. 15	23	51·	29·80	Cloudy
16	24	44·5	29·20	Fine
17	25	54·5	30·11	Ditto
18	26	51·	30·05	Ditto
19	27	48·	30·15	Ditto
20	28	48·	29·60	Cloudy
21	new	46·	30·22	Fine
22	1	45·	30·05	Ditto
23	2	46·5	30·02	Ditto
24	3	53·5	29·85	Ditto—brisk wind
25	4	53·	29·93	Ditto—ditto
26	5	52·5	29·75	Ditto
27	6	46·	30·35	Ditto
28	7	45·	30·40	Ditto
Mar. 1	8	45·5	30·03	Ditto
2	9	48·5	30·05	Cloudy
3	10	53·5	30·05	Ditto
4	11	53·	29·70	Fine
5	12	52·	29·67	Ditto—rain A.M.
6	13	52·	29·20	Cloudy—stormy with rain A.M.
7	full	42·	29·16	Stormy—hail and rain A.M.
8	15	40·	29·15	Rain
9	16	45·5	29·45	Ditto
10	17	53·	29·22	Stormy—rain P.M.
11	18	44·5	29·60	Ditto
12	19	46·	30·15	Fine
13	20	51·	29·90	Ditto
14	21	55·	29·68	Cloudy

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand.

Days of Month. 1822.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Weather.
Feb. 26	46	50	40	30·16	Cloudy
27	37	47	37	·68	Fair
28	33	48	35	·65	Fair
Mar. 1	32	47	40	·27	Fair
2	45	53	42	·36	Fair
3	42	57	45	·32	Fair
4	42	54	46	·06	Fair
5	46	54	50	·04	Fair
6	54	55	47	29·64	Stormy
7	46	48	35	·46	Stormy
8	36	47	45	·44	Rain
9	46	50	50	·74	Rain
10	50	54	44	·60	Stormy
11	43	47	37	[illegible]	Showery
12	35	48	42	30[illegible]	Fair
13	38	54	47	[illegible]	Fair
14	48	56	50	·01	Small rain
15	39	35	46	·25	Fair
16	47	54	50	·16	Cloudy
17	50	50	50	·30	Small rain
18	47	51	48	·44	Cloudy
19	50	60	54	·33	Cloudy
20	50	55	47	·33	Cloudy
21	47	57	46	·25	Fair
22	46	55	45	·40	Fair
23	39	60	46	·20	Fair
24	46	50	40	29·64	Small showers
25	40	49	39	·75	Showery
26	45	57	47	30·10	Cloudy

N.B. The Barometer's height is taken at one o'clock.

LI. *A curious electro-magnetic Experiment by* P. Barlow, *Esq. Royal Military Academy. In a Letter to the Editor**

To Dr. Tilloch.

Dear Sir,—Although I am not aware that the following electro-magnetic experiment will throw any additional light upon the very interesting results of Mr. Faraday of the Royal Institution, yet it is so very peculiar in the nature of its effects, and so pleasing in the exhibition, that it may be interesting to some of your readers, who have the means of repeating it. The machine is represented in Fig. 4 (Plate IV). AB is a rectangular piece of hard wood; CDE a piece of stout brass or copper wire; and *abcd*, a rectangle of smaller copper wire (soldered at E); on the lower side of which the wheel W of thin copper turns freely: *fg* is a small reservoir of mercury sunk in the wood; and *gi* a narrow channel running into it. HM is a strong horse-shoe magnet.

Mercury being now poured into the reservoir *fg*, till the teeth of the wheel are slightly immersed in it, and the surface covered with weak dilute nitric acid, make the connexion with the battery at *i* and D; and the wheel W will immediately begin to rotate with an astonishing velocity, far beyond the power of the eye to follow, and will thus produce the most pleasing effect.

The galvanic apparatus which I employed to produce this motion was the calorimotor of Dr. Hare which I had made of the plates of my old battery, 20 of zinc, and 20 of copper, each 10 inches square. But a much less powerful combination will be sufficient.

The suspension of the wheel is shown in fig. 5, and it may be proper to add, that in order to ensure a complete contact, the two sockets and the ends of the spindle should be amalgamated, as also the tops of the points of the wheel.

If the contact be changed, or if the magnet be reversed, the motion of the wheel will be reversed also; but I find the best effect produced when the wheel turns inwards.

Another curious experiment, and that on which the above is founded, is as follows:

After having been repeating Mr. Faraday's rotating experiment, the young man who was assisting me wished to try the effect of the horse-shoe magnet upon the freely suspended galvanic wire, as it hung with its lower end in the mercury. The wire was immediately thrown into a rapid oscillating motion,

* The Editor is happy to inform his readers that Mr. Barlow is printing a second edition of his Essay on Magnetic Attractions, which will also embrace the interesting subject of Electro-Magnetism.

flying completely out of the mercury; when the contact being thus broken, it fell by its own gravity to be again projected, and so on, as long as the action of the battery lasted.

The name of the young man alluded to above, is James Marsh, a very ingenious workman employed in the laboratory of the Royal Arsenal, who has constructed for me my calorimotor, and most of the other apparatus I have had occasion for in my experiments. It is much to be regretted that he is not in a situation to allow of a further and more profitable exercise of his ingenuity.

I remain, dear sir, yours very truly,

P. Barlow.

Royal Military Academy,
March 13, 1822.

LII. *On the Combination of Chrome with Sulphuric Acid.* By Gay-Lussac *

When dilute sulphuric acid in considerable excess is boiled upon chromate of lead or of barytes, the chromic acid is never pure. It always retains much sulphuric acid, even when ten times as much acid is used as is required to decompose the chromate employed. When the liquid containing the two acids is submitted to successive evaporations, it totally crystallizes in small quadrangular prisms of a deep red: if the concentration and heat are excessive, the chromic acid is partially decomposed, and the result is sulphate of green oxide of chrome. These crystals are very soluble in water, even deliquescent, and consist of an atom of sulphuric acid and an atom of chromic acid. They are analysed thus: Boil them with a mixture of muriatic acid and alcohol, to decompose the chromic acid, and change it to green oxide; then divide the liquid into two equal parts, precipitate one of them with muriate of barytes, to estimate the sulphuric acid; and the other with ammonia, to obtain the oxide of chrome; whence the quantity of chromic acid may be inferred. This compound of the two acids may be obtained at once by mixing them in a concentrated state; when a red precipitate immediately falls down. Nitric acid does not appear to adhere with any force to chromic acid, nor do they crystallize together, as with sulphuric acid.

The compound of chromic and sulphuric acid is readily dissolved in alcohol; but if the solution is concentrated, the reciprocal action of these substances takes place with a violence almost explosive. The chromic acid passes to the state of green oxide, and the liquid acquires a peculiar ethereous smell, similar

* From the *Annales de Chimie et de Physique.*

to

to that produced by treating peroxide of manganese with alcohol and diluted sulphuric acid. On distillation of either of these mixtures, and subsequent rectification over muriate of lime, to keep down any undecomposed alcohol, an ethereous liquid is produced, with an acid taste, and a penetrating smell like that of acetic ether, which on mixture with water separate into two strata, one of sulphuric ether, and the other of oil of wine.

When alcohol is distilled with sulphuric acid, and the addition either of chromic acid or of peroxide of manganese, it appears to undergo the same alteration as with sulphuric acid alone: sulphuric ether and oil of wine are formed by means of the oxygen of the chromic acid, or of the oxide of manganese. The sulphuric remains unchanged, but its presence is necessary to determine the decomposition of the alcohol, and the partial disoxygenation of the chromic acid, or of the oxide of manganese, by reason of its affinity with the oxides of chrome or of manganese. It should be remarked that Scheele had already observed, that in leaving together for some days a mixture of peroxide of manganese, sulphuric acid, and alcohol, and then distilling with a gentle heat, the alcohol passes over with a strong odour much resembling that of nitrous ether. M. Dobreiner has also remarked a similar odour in a mixture of chromate of potash, sulphuric acid, and alcohol, which he seems to attribute to a peculiar kind of ether produced by the action of chromic acid on the alcohol.

LIII. *On the Perspiration alleged to take place in Plants.* By *Mrs.* Agnes Ibbetson.

To Dr. Tilloch.

Sir,—Do plants perspire, or do they not? A careful examination enables me to say that no such property can be discovered to attach to plants.

When I first collected the many subjects I intended publishing on Botany, I divided the various parts into separate laws taken from the dissection of the vegetable in general. I thought Nature herself (as I proceeded progressively) seemed to arrange them thus: Mine was no hurried work, but one which has taken above twenty years to regulate, and is perfectly original, though entitled *Botany*. When I first introduced my dissections in Nicholson's Journal, near fourteen years past, I dissected a flower and corolla to show the curious manner in which a sort of flower or pattern was to be viewed when the various layers of the petal were seen together. I have not forgotten the joke that

was made on the occasion, though a well known and much esteemed astronomer, who had condescended to look over it, should have saved me from such an attack. Now nearly the same figure appears in the Linnean Transactions, and is acknowledged to be correct. I shall just insert my flower to show how easy it is to be prejudiced by the name of the presenter. Fig. 1 and 2.

As I divided my book into several different laws, and that on Perspiration was one of the first, I shall once more introduce the subject, having much more new matter and new reasons to offer against it. I shall first notice that Hales, who was, I believe, the first discoverer of perspiration in plants, lived at a time when the consideration of that apparent fact could *scarcely admit* of the sort of *examination it required.* Since that time so great has been the *alteration* established in our knowledge, that the decomposition of water, the acquaintance with the variety of gases, the condensation of the atmosphere, would alone demand a new arrangement. Now we know so much more of the regulation of the atmosphere, we are more capable of *reducing* the facts into natural *phænomena,* and bringing them *nearer truth.* Hales supposed that when a plant was covered it exuded a quantity of water. Indeed in the *then state of knowledge* he had every reason to believe it so, since he found the water within the glass running down at the interior, and often the appearance of bubbles of water on the leaves. As to his drawing sap from the vegetable by means of glasses, he only drew that which was hourly rising from the earth in sap; but he never applied a microscope to know whether *those bubbles* were *really water,* or the rose leaf alone would have immediately satisfied him of his error: nor did he place under the glass another object, which would have pointed out to him his mistake, since he would have discovered that an almost dry *sponge* would give nearly as much water as the plant, a very diminutive quantity of *which water* could have proceeded from either, as the sponge would evaporate full as much as the plant. But this is very different from the loads of water, nineteen times more than a healthy man exudes in perspiration. Had this been really the case, no person could have sat under a tree without being completely wet. However, allow for the general exaggeration, and suppose it only half the quantity, no microscope had been directed to the *plant,* these apparent bubbles had never been *examined,* nor even touched with the finger, which would quickly discover that they were not uncovered bubbles of water, but a species of chemical glass: but this is not the only object of our research; to discover from whence that water flows which appears under

a glass

a glass when a vegetable is placed there, is the chief question: Does it really proceed from the plant?

To ascertain this fact, and to prove the truth beyond all contradiction, has been the work of years of my life: to try the cause, I placed a stone instead of a plant, and the effect was nearly the *same;* a moist sponge was also inserted; but there is a still more convincing trial. A friend of mine has a large skylight, and he was lamenting the necessity of putting a glass within and a trough for carrying off the water which flowed; though no aperture was discoverable, still the stairs were inundated; *they could not perspire;* the water therefore must have proceeded from the condensation of the atmosphere without, as it does in an empty room when the water is often perceived running down the glass window though no one is within: still resolved not to fail for want of experiment, I tried what appeared to me an unanswerable one. I placed two plants in separate glasses of the same size: I covered one with a large cylinder; the other remained uncovered, except by the first glass: that which had two glasses has water (as in the skylight) running down the interior of the outward cylinder; no moisture, or a most trifling quantity, will be discovered where the plant is, as, when collected, it at no time gave two drops, the mere evaporation of the vegetable; but the other ran down a large quantity at the interior of the cylinder as usual. I know not how I can add more convincing experiments; the last I have tried repeatedly.

There can be no doubt, I should suppose, that the difference of caloric within and without the cylinder should cause some difference in moisture. The earth which evaporates so violently must greatly increase the water discovered within the frames of the Cucumber and Melons. But all this, if duly considered, and the great discoveries of *Priestley* and *Ingenhousz*, since Hales wrote, must cause new ideas, new conclusions: I merely lay my crude notions before the public, and submit them to more learned judges. But I cannot help adding, that had our trees perspired as botanists inform us they do, our trees, instead of the beautiful figure they make, would have appeared a mass of filth.

Bonnet (the most exact French botanist that country possessed) says he is persuaded that the leaves are garnished with organs for absorbing nutriment, which pass from them into the leaf: but unfortunately, instead of seeking with a microscope for those hairs or instruments which Nature has bestowed on the leaves, he sought them only by other experiments, microscopes being then not used on botanical occasions, or not good enough to satisfy him. Du Hamel expresses himself as perfectly convinced that absorbing organs of the leaf exist, though his microscopes are too feeble to enable him to judge of them thoroughly.

But,

But, what is most wonderful, they both arrive at the proper result, though by different means: since they both tried the experiment of placing a plant without water, without earth, but in a very moist atmosphere, and the plant has remained alive, nay, increased in weight, while another vegetable placed by its side in the same circumstances has died directly,—proving that the first absorbed all its nutriment from the atmosphere, and had no radicles to its roots; and that the other was an earthy plant receiving its nourishment from the ground and through radicles alone.

But the hairs are not designed for absorption only, or one sort of form would have sufficed: whereas in each vegetable there are often many different-shaped figures, continually accompanied with valves, and various species of mechanism of the most curious kind. There are two sorts of these hair-like figures or instruments in the Sweet Pea; two in the *Œnothera;* three in the white *Antirrhinum;* three in the *Lamium album*, and one admirable for its mechanism made nearly the same as in the Nettle, but without its poisonous juices. Nature never multiplies her means when one end only is in view and is to be answered. Will not then common sense explain the design Nature proposes in thus multiplying the figure of the hairs; or rather in forming different instruments in the same plant, and conclude these sorts of hairs were intended to effect a change in the juices entered within them, and produce that alteration the species of plant may require, and to confirm and establish that result? The hairs may be seen to change the colour of their juices after entering a vapour or sort of cloud which gets secreted between the valves; the hair afterwards admits another juice, and these coming into contact, *mix* and *explode*, and *produce* the alteration *required*. This is always the case with those instruments which compound the oil for the vegetable, and which are quite of a different shape from all other hairs, as in the Sun-flower plant, &c. Why all this mechanism and variety of forms, if it was only to admit a single juice, and that no alteration was to be effected by the liquid by means of the instrument? A simple pipe would have done *as well*. Numbers of my friends have seen this phænomenon; men of the greatest ability will testify to its truth, if seen in the compound microscope, and they have seen the hairs afterwards twist or flatten, and all the juices forced down into the leaf. In the Rose, the liquid enters the hair, the tint of water; and if it is watched for an hour or two, a thin vapour will be seen to insinuate itself and mix with the other juice: in a short time they explode; and so violent is the effect produced that much of the liquid is thrown out of the ball, and may be seen scattered all around: at last all subsides, and the liquid becomes a deep red. I remember showing it in

this

this state to Sir W. Herschel as one of the figures taken for perspiration, and he turned from me almost indignantly, saying that no one could take that for perspiration. But when I showed him the white balls and convinced him they were the same by *some half changed*, he altered his opinion, and was forced to touch them to convince himself they were not uncovered bubbles of water.

How wonderful is it to see that these diminutive delicate forms containing all the dangerous mixtures our strongest glasses can hardly endure, should yet bear the wind and weather, and never burst with the frost, though full of liquid! I have often taken them when the vegetable has been much injured by a sudden frost, to examine the hairs; and though they proved in general half empty, yet they had none of them burst, but the juices are often seen running up and down the hairs in a strange agitation. They often boil over indeed, but never break.

How astonishing then to see this amazing thin matter more like gauze, but of a strength that would bear the attack in which our thickest glass would fail! I cannot help adding a figure of one of the hairs to show the valves, fig. 3.

Figure 1 and 2: the view of the single *Anemone* to show how those sprigs found scattered in every part of the interior of the plant are also to be discovered in the corolla; in some they may be seen through the light if examined with a double microscope: in others it is only by stripping off the upper cuticle. Fig. 9 is the cylinder to which the stamens are fastened, and which passes between the numerous pistils and corollas. I have taken it from thence to show that each ingredient, that is, the stamens, pistils, and corollas, have the same sort of cylinder in all flowers whatever; but in the compound the cylinders pass, as in this, as low as *a a a*, where the mechanism *b b* is discovered in the plant, and where the different ingredients mount.

I must mention a few words respecting the atmosphere. I have shown that plants exercise by their leaves and roots a force of suction prodigiously great; but if they perspired, it would be returning to the atmosphere all that they had thus gained, since they would hardly absorb more than Hales assures us they give out. Where then would be the advantage of the suction, since they are supposed immediately to refund what they have taken? The absorption, as I have detailed, is made either by the hairs (as in sand plants) or in the upper surface of the leaves (as in rock plants), and the fluids thus received pass through the stem of the leaf and form the bark juice. Mirbel says that the perspiration and absorption cannot come from the same organ: he had just before said that he was convinced absorption came from the leaves, then the leaves cannot perspire.

It

It is certain that the sun having thus drawn the dews and moisture into the leaves, their hairs absorb and decompose it, and the sun thus reduces it to vapour. It is the light which produces this phænomenon much more than the caloric. I have often seen the water decomposed through the thin cuticle, just as it is done by the galvanic trough in the machine: but the light must be pretty strong.

LIV. *Observations on Magnetism: extracted from a Letter to Mr.* C. RUNKER, *of Hamburgh, from Professor* HANSTEEN, *of Christiana**.

WITH the little oscillatory instrument, which you saw at my residence in London (consisting of a magnetized steel cylinder, suspended by a very fine silk thread, and inclosed in a glazed case), I observed here, at Christiana, in the months of November and December 1819, and in March, April, and May 1820, seven or eight times every day, the time of 300 oscillations, by which I have found:

First; that the magnetic intensity of the earth is subject to a diurnal variation, so that it decreases, from the first hours of morning till about ten or eleven, when it arrives at its *minimum*; from that time it goes on increasing till four in the afternoon, and, in the latter months, till six or seven in the evening. This force afterwards decreases anew during the night, and about three in the morning reaches its *maximum*; whence it again returns, by little and little, to its *minimum* about ten or eleven in the morning, and so on continually.

Second; that whenever the moon passes the equator, the magnetic intensity is considerably weaker in the two or three following days.

Third; that the magnetic intensity is still more reduced, during the appearance of an *aurora borealis*, and is so much the weaker as this meteor is extensive and powerful. The common intensity returns only by degrees, and 24 hours afterwards.

Fourth; that the magnetic intensity appears to have a very considerable annual variation, being stronger in the winter months than in the summer months.

When the magnetic cylinder makes 300 oscillations in 813·6 seconds of time, I assume the corresponding intensity = 1·0000, and, as the intensities are in the inverse ratio of the squares of the time of the oscillations, we can always express, in these supposed parts, every intensity answering to the times of the oscillations. It is according to this supposition that I have observed and found the results which I offer here in the following tables.

* From Zach's *Correspondance Astronomique, Géographique, Hydrographique et Statistique.*

December 1819.

Hours.	9 in Morn.	10 to 11 Morn.	12, 1, 2, Aft.	3, 4, 5, Aft.	6, 7, 8, Aft.	10, 11 Even.	Mean.
Mean Intensity	1·01931	1·01902	1·01915	1·01966	1·01929	1·01732	1·01912

The greatest force, December 14, at 3 in the afternoon .... 1·0242 time of oscill. 803″·90
The least force, December 16, at 11 in the evening .... 1·0082 time of oscill. 810 ·31

Difference .. .. = 0·0160 Diff. .. = 6·41

March 1820.

Hours.	8 Morn.	10 Morn.	Noon.	2 After.	4 After.	6 Even.	8 Even	10 Even.	Mean.
Mean Intensity	1·01095	1·01010	1·01023	1·01136	1·01147	1·01113	1·01142	1·01063	1·01081

The greatest force, March 5, at 10 in the morning .. .. 1·0174½ time of oscill. 806″·58
The least force, March 29, at 10 in the morning .. .. 1·0070½ time of oscill. 810 ·74

Difference .. .. = 0·0104 Diff. .. = 4·16

April 1820.

Hours.	8 Morn.	10½ Morn.	4 After.	7 Even.	10 Even.	Mean.
Mean Intensity	1·00717	1·00625	1·00879	1·00966	1·00903	1·00818

The greatest force, April 27, at 7 in the evening .. .. 1·0151 time of oscill. 807″·53
The least force, April 3, at 10½ in the morning .. .. 1·0039 time of oscill. 811 ·98

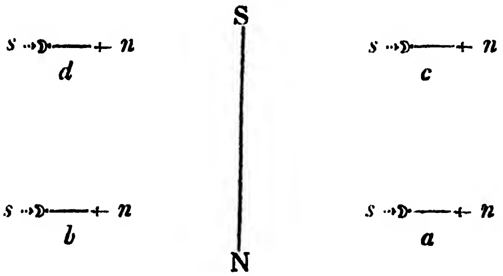
Difference .. .. = 0·0112 Diff. .. = 4·45

Hence

Hence it results that the intensity in Dec. 1819 = 1·01912
Mar. 1820 = 1·01081
Apr. 1820 = 1·00818

In the month of May, in which I have not yet completed the observations, the force has a little diminished: I suspect that it will increase when the earth shall have passed the aphelion.

My second magnetic discovery is the following: I have found that every vertical body S N, whatever it be, and of any kind of matter, has a North-pole at bottom and a South-pole at top, as in all vertical bars of iron.



For otherwise it would be impossible to explain the phænomena which I have observed, and which I have determined by a great number of incontestable experiments; namely, that the magnetic cylinder oscillates more quickly towards the North in *a*, and more slowly towards the South in *b*. And, on the contrary, it oscillates more slowly towards the North in *c*, and faster towards the South in *d*. I have found this law constantly confirmed by my experiments near the walls and partitions of houses, whether of wood or stone, and even near large trees in the gardens. This action must necessarily exert its influence, indeed considerably, on the direction of the compass-needle on shipboard. The whole mass of wood in a ship has, in this way, a magnetical axis, and the observed variation of the compass ought rather to be attributed to this influence than to that of the iron, guns, and ballast, carried by the vessel. Hence it results, that all observations on the magnetic intensities made within doors are uncertain.

A Table of the actual Intensity of the Magnetic Force in different Parts of the World, calculated from a great Number of Observations, by the ingenious and laborious Professor Hansteen. (Zach.)

Places.	Dip.		Intensity.
Peru	0°	0′	1·0000
Mexico	42	10	1·3155

Places.	Dip.		Intensity.
Paris	68°	38′	1·3482
London	70	33	1·4142
Christiana	72	30	1·4959
Arendahl	72	45	1·4756
Brassa	74	21	1·4941
Hare Island	82	49	1·6939
Davis's Straits	83	08	1·6900
Baffin's Bay	84	25	1·6685
	84	39	1·7349
	84	44	1·6943
	85	59½	1·7383
	86	09	1·7606

LV. *Reply to Mr.* H. B. Leeson. *By* J. Murray, *F.L.S. M.W.S. &c. &c.*

To Dr. Tilloch.

Sir, — I have neither time nor inclination for any thing controversial, and least of all do I wish to disturb Mr. Leeson's tranquillity, in reference to *his* "Safety Appendages" to Toft's Hydrostatic Blowpipe. My remarks therefore on his last paragraph, which includes notice of me, shall be succinct.

The use of *mercury* is conceded to me as recommended on the plan of Marquis Ridolfi; but it should seem either that I had omitted to state the necessity of a cell to contain it, or was ignorant that *iron* alone was proof against the action of quicksilver—*Credat Judæus apella.* The following are the words used by Mr. L. in a letter to me, dated 18th January last: "You told me that *mercury* had been adopted (*employed?*) by the Marquis Ridolfi, and that you thought it preferable to oil or water; on which I observed that the cylinder must in that case be made of iron!"

When at Florence, this interesting young nobleman was good enough to sketch with his own hand, though labouring under a severe accident, the consequence of chemical experiment, the attachment to the gas blowpipe, to which the preceding refers. You had the kindness to insert in your pages a copy of this sketch and its description. Mr. Leeson might have there seen this described as of *iron.*

Mr. Toft's blowpipe was constructed at Nottingham, under Mr. Leeson's directions; when finished, the instrument was charged with an explosive atmosphere, and at the orifice of Dr. Hope's Safety Box of Wire-gauze (certainly proof against all explosion, nor can I too warmly recommend its use) the gaseous

mixture burnt tranquilly. It was unscrewed, and the gas ignited at the extremity of a capillary tube; the flame receded, an explosion ensued, and the instrument was destroyed. Mr. Leeson's "valve therein" was *a common button valve.* This is the circumstance which Mr. L. would inform us of at page 403 of your December Number.

Having said thus much for the safety cistern, I shall now advert to the "valve therein."

From the explosion adverted to, which Mr. L. ascribed to the valve being rudely made, I concluded that some *other plan* of the valve was advisable, though Mr. Leeson thought the same valve repaired would do. The plan I proposed is now introduced in Mr. Leeson's own words, quoted also from his letter of 18th January. It was submitted before Mr. L. to one Andayna for such alteration and improvement as he saw necessary.

"Your plan of two button valves, to be connected together by a solid spindle up the sides of which the gas was to pass, and which were to be rendered air-tight by two pieces of leather attached to their under surfaces, and the buttons were to be prevented from rising too high by two small bits of wire inserted above the upper valve." This *may* perchance be pronounced not a "modification" of that to which we find "H. B. Leeson invt. et del." attached. But there is no doubt of its being equally safe—by this provision of a double guard both valves closing simultaneously.

Allow me to ask, sir, why this anxiety to entertain us with different arrangements of the "Safety cistern and valve therein?" The bundle of wires deposited in the cell serves all the purposes of the wire-gauze with which Mr. L. *now* crams the cistern; and under such circumstances, which is merely placing Dr. Hope's wire-gauze box *within* the cistern, instead of exterior to it, the instrument would be safe *without any valve at all;* aye, or even *mercury.*

I own that I was much amused with "experiment" and "explosion," nay, "repetitions" of them, so loudly vaunted in Mr. Leeson's "new observations;" being sadly sceptical, whether coming from this young gentleman, I am to regard them *vox et præterea nihil.* Mr. Leeson in a letter to me (13th November) advised "a good way of trying the Safety Appendages," —"to connect them with a bladder containing the explosive mixture, set a candle before the jet and open the cock of the jet-pipe by a long string!" Are we to understand that his "experiments," "explosions," and "repetitions," were made in this manner?

I honestly confess that I am sorry for having written the note

annexed

annexed to Mr. Leeson's paper, and thus to ruffle his quiet. It must however be obvious that *I* had no interest in doing so. The iron cistern and mercury belong to Marquis Ridolfi; the cane and fasces of wires belong to Dr. Clarke and yourself; the multiplied folds of wire-gauze to Dr. Hope, and the valve *say* to Mr. H. B. Leeson.

I have the honour to be, sir,

Your obedient humble servant,

J. MURRAY.

LVI. *Comparison of the Expense attending the English and Scotch Systems of Husbandry. By Mr.* ANDREW SCOTT, *of Ryden's Farm, Walton-upon-Thames* *.

I HAVE the honour of presenting to the Board of Agriculture some statements on the œconomy of the Scotch system of farming, which I practise, and proceed to state the difference of expense between the English and Scotch modes of farming. The first circumstance which I have to notice, is, that my ploughing is performed with two horses, instead of three. This, besides saving the keep of a horse, also saves the expense of a boy, an appendage always required when three horses are put to a plough. The keep of a horse cannot be stated at less than 45*l.* per annum; and a boy at 5*s.* a week, is 13*l.* But from the boy being sometimes employed in harrowing, driving dung, &c. in which cases a boy is also required upon the other system, it would be unfair to charge the full amount of his wages; 8*l.* however, out of the 13*l.* I think, ought to be calculated upon, which, with the sum charged for the keep of a horse, makes 53*l.*; and as on the rotation I follow, a plough cannot manage more than fifty acres, a saving is thereby gained of a trifle more than 21*s.* per acre. It may be added, that my lands, as well as most of those in this neighbourhood, consist of a sandy or hazel loam, and such as two horses, at all seasons, are abundantly able to plough; but there are clays in this county, where four and six horses are put to a plough, and where two would be altogether insufficient, particularly in the summer season, when they are baked with the drought. I however think, that, by adopting the use of another plough, the number of horses may be reduced at least one-third, and during a greater part of the year one-half.

A measure very properly connected with the two-horse plough is the using of one-horse carts, instead of those in general use requiring three horses. With the latter a greater weight than

* From Communications to the Board of Agriculture.

30 cwt. is seldom taken; whereas 15 cwt. is a moderate load for the former, thereby making two horses do the work of three. Though in journeys this is the proportion, yet in the work upon a farm, such as manuring land, &c. it is still more; as from the greater facility the one-horse carts afford in filling and emptying, three horses in this way will often, when roads are good, do as much work as six in the other way, that is to say, with three to a cart. One advantage, however, which the English system possesses over the Scotch is, that by the additional horse more work can be done in harrowing; but that advantage is fully counterbalanced by what has just been stated regarding the one-horse carts. Besides, it has been invariably found that two horses, placed abreast in the plough, will get over more ground than three put in a line. This arises from their turning quicker, and being more free and disencumbered in walking.

The using of machinery for thrashing and dressing corn, is what I have next to notice. The one I use is of too small a size, but one a little larger, and of a proper construction, will thrash 12 quarters of wheat, and 18 of barley and oats, per day. The wages of the people employed in doing this, amount to 12*s.*; four horses at 3*s.* each, 12*s.*; dressing with hand-machine 5*s.*; and interest at 10 per cent. on cost of machinery, 7*s.*; making a total of 36*s.* or 3*s.* per quarter for wheat, and 2*s.* for barley and oats. The money given for wheat hand-thrashed, is about 6*s.* per quarter, and barley and oats 3*s.* If 3½ quarters per acre of the former grain, and 6 of the two latter, are stated to be average crops, on land worth 50*s.* per acre, there will then be a saving of 10*s.* 6*d.* an acre on the first, and 6*s.* on the latter; and as I calculate upon having one-third of my lands in wheat, and one-sixth in barley and oats, the saving on these crops by machine-thrashing will be 9*s.* per acre, or 4*s.* 6*d.* on the whole farm. Perhaps the charge for horse labour may be thought too low; but when it is recollected that thrashing is generally done in wet and frosty weather, when horses often cannot be employed in other work, it seems fair only to charge a trifle more than their keep. Another advantage attending machine-thrashing is, that grain can be brought to market at any time, thus enabling the farmer to avail himself of any sudden advance in price. Besides, it has been pretty satisfactorily ascertained in Scotland, that a twentieth part more grain will be got when thrashed with a proper machine, than when done with the hand. Though in many cases, particularly when wheat is blighted, I am satisfied there will be fully that difference here, yet when grain is well ripened, it certainly is not so much; though it might be observed, that when grain is to be hand-thrashed, it requires to stand longer on the ground than is necessary for machine-thrashing, and

and consequently a greater loss is sustained from shaking by wind, as well as in the process of reaping. This circumstance, together with what has been stated respecting the thrashing, I have little doubt will make a difference of five per cent. in the produce. A further advantage attending the thrashing-machine is, that it prevents pilfering by labourers; a circumstance of no small importance, as it is generally believed in this quarter, that farmers are injured a good deal in that way when corn is hand-thrashed. After having stated so much in favour of the thrashing-machine, I have now only one objection to state against it. In this situation, straw is an article of some profit, and by machine-thrashing, its price is reduced, but that disadvantage of course would cease, were they to get into general use.

That the difference betwixt the system of cropping, which I have laid down for my lands, and the rotation most common in this neighbourhood, may also be shown, I shall now add an estimate of the annual expense and produce of a farm of 210 acres, tithe free, under each rotation. My rotation is, 1st, turnips (drilled); 2d, barley, or oats; 3d, clover; 4th, wheat, after which, autumn or stubble turnips; 5th, potatoes; 6th, wheat; after which, part rye and part tares, to be fed on the ground, or cut for soiling. The other, or the common rotation in the neighbourhood, is, 1st, turnips (broadcast; 2d, barley; 3d, clover; 4th, wheat, after which, part stubble turnips; 5th, oats, after which, part rye for sheep-fed, say one-half. Before proceeding further, it may be necessary to premise, that in this situation, at least 10 per cent. of the ground is occupied with hedges, ditches, roads, farm-buildings, &c.; but from 5 per cent. being sufficient for these purposes, when fields are a suitable size, with hedges and ditches of a proper description, I have only deducted ten acres, thereby leaving 200 for crops. This divided by six, the number of years in the first rotation, gives $33\frac{1}{3}$ acres for each crop, and divided by five, gives 40 in the second. From the larger proportion of green or fallow crops in the former, a team, that is, a man and two horses, are charged more than in the latter, it being assumed the difference of horse labour in the two rotations is equal to one-fourth.

Cost of Horses, Implements, &c. and Annual Expense of the First, or Six Years' Rotation.

	£.	s.	d.
Eight horses, at 40*l*.	320	0	0
Harness for ditto	42	0	0
Eight carts (with frames), at 16*l*.	128	0	0
Five ploughs, at 4*l*. 10*s*.	22	10	0
Carried forward	£512	10	0

	£	s.	d.	£	s.	d.
Brought forward	£512	10	0			
Drills and drill-ploughs	18	0	0			
Rollers, harrows, drags	36	0	0			
Thrashing and dressing machines	120	0	0			
Sacks, sieves, bushel, ladders, shovels, spades, prongs, pails, mattock, axe, wheel-barrow, &c.	25	0	0			
	£711	10	0			
210 acres, at 50*s*. per acre				525	0	0
Poor-rate 2*s*. church-rate 6*d*. (per pound) ..				65	12	6
Property-tax 7½ per cent.				39	7	6
Assessed-tax on eight horses, at 17*s*. 6*d*. ..				7	0	0
Keep of eight horses, at 45*l*. per annum				360	0	0
Diminution of value, at 10 per cent.				32	0	0
Blacksmiths' work				30	0	0
Carpenters' or wheel-wrights' ditto				25	0	0
Sadlers' or collar-makers' ditto				10	0	0
Four ploughmen, at 16*s*. per week				166	8	0
Boy, at 5*s*. per week				13	0	0
Ditto, at 3*s*. 6*d*. per ditto (to keep rooks off crops, &c.)				9	2	6
Extra man for fencing, rick-thatching, hay and straw binding, harvest-work, &c.				41	12	0
66⅔ acres land, cleaning with hand, at 3*s*. ..				10	0	0
33⅓ acres potatoes cutting and planting, at 7*s*.				11	13	4
Ditto twice hand-hoeing, at 8*s*.				13	6	8
Ditto taking up, at 40*s*.				66	13	4
Extra hands for storing and measuring ditto for sale				8	0	0
33⅓ acres drilled turnips, hoeing, at 10*s*. ..				16	13	4
Ditto, barley and oats, reaping, at 13*s*.				21	13	4
Ditto, clover, twice mowing, at 8*s*. 6*d*.				14	3	4
Ditto, making and stacking, at 11*s*. 6*d*.				19	3	4
66⅔ acres wheat, reaping, at 15*s*.				50	0	0
Extra hands for dung, filling and spreading, ditching, corn harvesting, &c.				24	0	0
Ditto for working, thrashing, and dressing-machine				8	0	0
Incidental expense				15	0	0
Seed for 33⅓ acres turnips, at 1*s*. 6*d*.				2	10	0
Ditto ditto barley and oats, at 20*s*.				33	6	8
Ditto ditto clover, at 14*s*.				23	6	8
Ditto for 66⅔ ditto wheat, at 30*s*.				100	0	0
Ditto for 33⅓ ditto potatoes, at 40*s*.				66	13	4
Ditto ditto stubble turnips, at 2*s*.				3	6	8
Carried forward				£1841	12	6

		£	s.	d.	£	s.	d.
Brought forward					£1841	12	6
Seed for $16\frac{2}{3}$ stubble rye, at 15*s.*					12	10	0
Ditto ditto tares, 30s.					25	0	0
Manure for 60 acres, at 5*l.* 5*s.* per acre					315	0	0
Annual expense		£2184	2	6	£2184	2	6
Add the first cost of horses, implements, &c.		711	10	0			
Total capital and ann. expense		£2895	12	6			
The interest on this sum, at 5 per cent. is					144	15	$7\frac{1}{2}$
Which, added to the annual expense, amounts to					2328	18	$1\frac{1}{2}$
This divided by 210, the number of acres, gives the annual expense per acre at					£11	1	$9\frac{1}{2}$

Annual Produce.

	£.	s.	d.
$33\frac{1}{3}$ acres turnips, at 4*l.* per acre (fed on the ground)	133	6	8
Ditto ditto barley and oats (five quarters per acre of former, at 40*s.* and $6\frac{1}{4}$ of latter, at 32*s.*) 10*l.*	333	6	8
Ditto ditto clover (two crops, making $2\frac{1}{2}$ loads, at 5*l.*) 12*l.* 10*s.*	416	13	4
$66\frac{2}{3}$ ditto wheat ($3\frac{1}{2}$ quarters per acre, at 80*s.*) 14*l.*	933	6	8
$33\frac{1}{3}$ ditto potatoes (six tons of $23\frac{1}{2}$ cwt. at 4*l.*) 24*l.*	800	0	0
Ditto ditto stubble turnips, at 20*s.*	33	6	8
$16\frac{2}{3}$ ditto rye, at 1*l.* 10*s.*	25	0	0
Ditto ditto tares, at 5*l.*	83	6	8
80 loads of wheat straw sold, at 2*l.* 5*s.*	180	0	0
Divide by 210	£2938	6	8
Average annual produce per acre	13	19	10
Deduct expenses	11	1	$9\frac{1}{2}$
Annual profit per acre	£2	18	$0\frac{1}{2}$

Cost of Horses, Implements, &c. and Annual Expense of the Second, or Five Years' Rotation.

	£.	s.	d.
Nine horses, at 40*l.*	360	0	0
Harness for ditto	45	0	0
Four ploughs, at 5*l.*	20	0	0
Carried forward,	£425	0	0

				£	s.	d.
Brought forward	£425	0	0			
A waggon	50	0	0			
Three carts, one small ditto ..	105	0	0			
Rollers, harrows, drags	36	0	0			
Sacks, sieves, screen, fan, bushel, shovels, spades, prongs, axe, pail, ladders, wheel-barrows, &c. ..	30	0	0			
	£646	0	0			
210 acres, at 50*s.* per acre				525	0	0
Poor-rate 2*s.* church ditto 6*d.* (per pound) ..				65	12	6
Property tax 7½ per cent.				39	7	6
Assessed tax on nine horses, at 17*s.* 6*d.*				7	17	6
Keep of nine horses, at 45*l.*				405	0	0
Diminution of value, at 10 per cent.				36	0	0
Blacksmiths' work				25	0	0
Carpenters' or wheel-wrights' ditto				20	0	0
Sadlers' or collar-makers' ditto				10	0	0
Three ploughmen, at 16*s.* per week				124	16	0
Ditto ploughboys, at 5*s.* per week				39	0	0
One boy, at 3*s.* 6*d.* (to keep rooks off crops, &c.)				9	2	6
Extra man for hay-binding, rick-thatching, harvest-work, ditching, &c.				41	12	0
Forty acres ground cleaning with hand, at 4*s.* ..				8	0	0
Ditto ditto turnips-hoeing, at 13*s.*				26	0	0
Ditto ditto barley-mowing, at 4*s.*				8	0	0
Cocking, raking, and stacking ditto, at 5*s.* ..				10	0	0
Forty acres clover, twice mowing, at 8*s.* 6*d.* ..				17	0	0
Making and stacking ditto, at 11*s.* 6*d.*				23	0	0
Forty acres wheat, reaping, at 15*s.*				30	0	0
Ditto ditto oats, mowing, at 4*s.*				8	0	0
Cocking, raking, and stacking ditto, at 4*s.*				8	0	0
Extra hands for dung, filling, and spreading, fencing, ditching, &c.				16	0	0
140 quarters wheat, hand-thrashing, at 6*s.* ..				42	0	0
200 ditto barley, ditto, at 3*s.*				30	0	0
Ditto ditto oats, ditto, at 3*s.*				30	0	0
Incidental expenses				12	0	0
Seed for 40 acres turnips, at 2*s.*				4	0	0
Ditto ditto barley, at 20*s.*				40	0	0
Ditto ditto clover, at 14*s.*				28	0	0
Ditto ditto wheat, at 30*s.*				60	0	0
Ditto ditto oats, at 20*s.*				40	0	0
Ditto for 20 acres stubble turnips, at 2*s.*				2	0	0
Ditto ditto rye, at 15*s.*				15	0	0
Manure for 30 acres, at 5*l.* 5*s.* per acre				157	10	0
Carried forward				£1962	18	0

		£.	s.	d.
	Brought forward	1962	18	0
Annual expense	£1962 18 0			
Add the first cost of horses, implements, &c.	646 0 0			
Total capital and annual expense	£2608 18 0			
The interest on which, at 5 per cent. is		130	8	11
Which, added to the annual expense, amounts to		2093	6	11
This divided by 210, the number of acres, gives the annual expense per acre at		£9	19	4½

Annual Produce.

	£.	s.	d.
Forty acres turnips, at 3*l.* per acre (fed on the ground)	120	0	0
Ditto ditto barley (five quarters at 40*s.*) 10*l.* ..	400	0	0
Ditto ditto clover (two crops making 2½ loads, at 5*l.*) 12*l.* 10*s.*	500	0	0
Ditto ditto wheat (3½ quarters, at 80*s.*) 14*l.* ..	560	0	0
Ditto ditto oats (5 quarters, at 32*s.*) 8*l.* ..	320	0	0
Twenty acres stubble turnips, at 20*s.*	20	0	0
Ditto ditto rye, at 30*s.*	30	0	0
Eighty loads wheat straw, sold at 45*s.* per load	180	0	0
Divide by 210	£2130	0	0
Average annual produce per acre	10	2	10¼
Deduct expenses	9	19	4¼
Annual profit per acre	£0	3	6

From the preceding calculations, it appears then, that upon the rotation first noticed, under the Scotch system of labour, there is a profit of 58*s.* per acre, while upon the second, under the English system, there is not more than 3*s.* 6*d.* per acre, being a difference of 54*s.* 6*d.*; and if to this be added 4*s.* 6*d.* as gained by the advantages attending the thrashing-machine, this will form the sum of 59*s.* per acre of profit, which the management that I have adopted affords more than that of this neighbourhood. As the profit, however, of 3*s.* 6*d.* per acre, additional to common interest on capital, is certainly smaller than farmers are known to get, it is necessary to notice some circumstances which contribute to their profits, that the estimate has

 not

not included. In this situation, most farms contain a considerable proportion of meadow or old grass land, and which, at the rent stated, produces a much larger profit than the tillage ground, under the management detailed. Besides, the minutiæ of a farm, here, are not inconsiderable; pigs, poultry, &c. all producing a profit. But what has contributed most to the farmers' profits of late years, has been the very high price of grain, the value of the corn crops, at present, being more than one-third higher than charged in the estimate; so that under all circumstances, I have no reason to doubt the accuracy of the calculations. At any rate, from the statements in both estimates being founded upon the same data, the result in both ought to be alike accurate. It is, however, to be observed, that owing to the profits of farmers being influenced by so many circumstances, it is impossible, by any calculations, to ascertain them exactly. A larger or smaller degree of skill and attention will make a considerable difference in the profit: besides, it is to be noticed, that in this quarter, farms when entered upon are generally in a very foul and impoverished state, and in consequence of this, crops are defective for some years; which, together with the improvements that may be necessary at that period, often occasions a loss of very considerable magnitude: interest, therefore, on the amount, must be deducted from the annual profits.

In forming the foregoing estimates, the greatest difficulty I have experienced, has been to ascertain the quantity of straw sold, and the amount of money paid for dung. In the first rotation, one-half of the ground is proposed to be dunged moderately every year, viz. turnips, clover seeds, and potatoes. It is supposed the $33\frac{1}{3}$ acres of barley and oat-straw, together with $26\frac{2}{3}$ wheat-straw consumed on the premises, in foddering cattle, littering horses, thatching of ricks, &c. will, with the profits on the stock foddered, produce dung for forty acres. This leaves sixty to be provided for, and which, at 5*l.* 5*s.* (the money at which the proposed allowance for an acre can be brought from London by water) will cost 315*l.* as charged in the estimate. But from forty acres, wheat-straw being sold at 4*l.* 10*s.*, 180*l.* are received towards the above sum, thereby leaving 135*l.* to be advanced yearly for this article. In the second rotation two-fifths of the ground are dunged yearly, viz. forty acres turnips, and forty clover seeds. Again, in this case, the straw of the eighty acres of barley and oats is supposed to produce, in the above way, dung for fifty acres, thereby leaving thirty to be supplied otherwise, and which, at 5*l.* 5*s.* per acre, amounts to 157*l.* 10*s.*; forty acres of wheat-straw, however, being sold at 4*l.* 10*s.* produce 180*l.* thus leaving a profit of 22*l.* 10*s.* on straw. It will be noticed, that in the first estimate, drilled turnips are charged

20*s.*

20*s*. per acre higher than the broadcast in the second; and this difference in favour of drilling, it is presumed, any one acquainted with the greater produce that is got in that way, will admit to be fair. To one not acquainted with the climate of this country, it may appear, that the obtaining of a crop of turnips after tares or rye cut for soiling, is impracticable; but when it is known, that the time most approved for sowing turnips here, is from the middle to the end of July, and that in this ease, the ground is supposed to be perfectly clean, there will then no longer appear any difficulty.

April 24, 1813.

Explanatory Letter from the same.

Sir,—Of the 60 acres of straw stated to be consumed at home, the 26⅔ acres of wheat are estimated at 3 loads of 11 cwt. 2 quarters 8 lbs. or 34 cwt. 2 quarters 24 lbs. per acre; and the 33⅓ acres of barley and oats, at 2½ loads, or 28 cwt. 3 quarters 20 lbs. This makes 163 loads, or 94 tons 6 cwt. 16 lbs. Besides, it was omitted to be noticed, that of the 40 acres of wheat-straw, sold at 4*l*. 10*s*. an acre, two loads only of the most marketable were supposed to produce that sum, so that 40 loads from this source are to be added to the above, thereby making 203 loads, or 117 tons 9 cwt. The quantity of dung applied to an acre, on an average, is 12 tons nearly, say 10 for turnips, 10 for clover seeds, and 15 for potatoes. Upon this calculation 40 acres will require 466⅔ tons, and which the above 203 loads of straw are supposed to produce in the following way: first, eight horses will require for litter two trusses per day, or about 20 loads per annum. This with the hay, clover, corn, &c. used by the horses, is estimated to produce 69 tons, or dung for five acres. Of the remaining 183 loads, it is supposed about 53 may be required for thatching of ricks, cows, pigs, &c. littering, and that the other 130 shall be used in foddering stock. The 183 loads used in this way, it is assumed, will produce at least 233 tons, or dung for twenty acres; and the profits on the stock foddered will procure the quantity required for the remaining fifteen acres. A load of straw will pay at least 12*s*. 6*d*. by taking cattle in to fodder, and this sum on 130 produces 81*l*. 5*s*. being fifty shillings more than is to purchase dung for fifteen acres at 5*l*. 5*s*. Having given this explanation of the first calculations on the above subject, I think it unnecessary to state any thing regarding the second, as the same observations are applicable to both. It may be proper to observe, that though I have reason to believe that the above estimate of 60 acres of straw, producing dung for 40 in the way stated, to be correct, yet as I have not had sufficient practice to prove it by the test of experience, I cannot pledge myself for its accuracy.

accuracy. However, from the calculations in both systems being founded upon the same data, it does not affect the comparative result. Yours, &c. ANDREW SCOTT.

Though the present communication was written so far back as the year 1813, we have no reason to believe that the system which it condemns has been at all improved. In fact, the English agriculturists were at that time rolling in wealth, from the extravagant prices then procured for their produce; so much so that any thing in the shape of an œconomical saving in the expenses of their business was beneath their notice. Or shall we speak plain truth, and say that they are, generally speaking, so ignorant, so wedded to prejudices, that hardly any thing will drive them from the system of their forefathers, however wasteful and stupid? In their present circumstances, however, it may be thought, when ruin stares many of them in the face, that they will be inclined to profit by the experience of others.

It is a circumstance deserving of particular notice at the present moment, when the distress of the English farmers is so general as to be avowed in loud complaints to parliament, from every county and almost every parish, that no complaints of this kind have been received from the Scotch farmers. This speaks volumes; and here it may not be out of place to notice a fact, stated in the County Herald of the 8th of March, which serves to prove that the practice of our farmers (at least of a great majority of them) continues the same as in the year 1813, when the above communication was made to the Board of Agriculture. In the paper alluded to of the 8th of March, it is stated, that an experiment "was lately tried, in order to ascertain the difference between the working of the long mouldboarded plough (used within 25 miles of London), with *four horses, a man and a driver*, and a common Scotch plough, with *a pair of carriage horses*, and reins. The result turned out, that *the pair of horses* ploughed, in six hours, *one* acre, *nine* inches deep by *twenty*, walking at the rate of *three* miles an hour; the *four horses* ploughed *half* an acre *seven* inches deep by nine, stepping *two* miles in an hour."—That is, where this wasteful system is pursued, eight horses are required to perform the same work that the Scotch farmer executes with two.

Our land-holders, who are fond enough of money, should turn their attention to facts such as this in granting their new leases. It is just as reasonable that the farmers should be tied to an œconomical mode of culture as to a regular rotation of crops. They have no right to subject the public to the extra expense of a wasteful mode of culture, when a more œconomical is not only recommended, but its advantages demonstrated in real practice.

EDITOR.

LVII. *On dilating Caoutchouc Bottles by Inflation.* By B. M. Forster, *Esq.*

To Dr. Tilloch.

Sir, — The great expansibility of the *Caoutchouc* or India-rubber is well known: but I am not aware that any endeavours have heretofore been made to inflate the bottles made of that substance, with air, with a view to enlarge their capacity. On Tuesday the 19th instant, I threw some air into a small bottle of it, with a condensing syringe, which caused a small blister (if so I may call it) on the lower part of the bottle; since which, by proceeding in the same way, the bottle was enlarged from *about* two inches and a half (diameter) to about six and a half. I do not know *exactly* the dimensions. The mode of the expansion is to me rather surprising: the globe did not expand in an uniform manner, but a blister was formed which increased from what may be called the bottom (if the term bottle is used) towards the neck, where the syringe was connected. I have this evening blown it up without a condensing syringe to very nearly six inches diameter. For some way below the neck the India-rubber retains its usual appearance, not being stretched out like the other part; which part has the look of an animal's bladder, full blown; or a globe of thin horn. I am of opinion that globes of this kind will in many respects be found preferable to bladders for philosophical and other purposes. If the expansion can be continued to a very considerable extent, I am in hopes that *air balloons* may be made with these globes. In two trials I have burst the bottles before the expansion was arrived at nearly the degree to what it was in the instance above mentioned.

It has appeared to me remarkable, when (warmed) paper has been excited with a piece of India-rubber, that the rubber showed very little signs of being electric, although the paper was *strongly* electrified. This *caoutchouc globe* when rubbed on paper (warmed) becomes strongly electric, and produces sparks attended with snappings.

Walthamstow, Essex, March 26, 1822. B. M. Forster.

LVIII. *On melting Caoutchouc, or India-Rubber, and preserving Iron and Steel from Rust.* By Arthur Aikin, Esq. *Secretary to the Society for the Encouragement of Arts, Manufactures and Commerce**.

19, John-street, Adelphi, Dec. 24, 1821.

Dear Sir,— You well know the many attempts that have been made to preserve iron and steel from rust, and the small success with which they have been in general attended. Greasy and oily, or resinous, substances have formed the basis of the different preparations proposed and employed for this purpose: but in the former, when rancidity comes on, an acid is produced which corrodes the iron; and the latter, when dry, are apt to crack, and thus afford an inlet to moisture, which, as soon as it has insinuated itself, begins to act on the iron, and to throw off the varnish in scales, on account of the enlargement of bulk which the particles of iron undergo when converted into oxide.

Some time ago the thought occurred to me, that melted caoutchouc would be found to possess peculiar advantages in preserving the surface of iron from being acted on by the atmosphere; arising from its little susceptibility of chemical change when exposed to the air; from its treacly consistence under all ordinary temperatures; from its strong adhesion to the surface of iron or steel; and at the same time from the facility with which it is removed by a soft rag and a piece of stale bread.

I accordingly made the trial, by procuring small plates of iron and of steel, and smearing one half of their surface lightly over with the caoutchouc, and exposing them on a table in a laboratory during the last five or six weeks. The result has been, that the portions of the plates covered by the caoutchouc have been preserved unchanged, while the unprotected portions have been almost entirely corroded. The finger or a soft brush are the most convenient implements for applying the caoutchouc; and, as soon as the article has been covered, it ought to be set up on end, in order that the excess may drain from it, which will take place in a day or two.

The temperature for melting caoutchouc is nearly equal to that required for the fusion of lead; but if this is attempted to be performed in a pipkin, or any other open vessel, a copious emission of vapour will take place, the mass will become more or less charred, and is very likely to take fire. I therefore requested my friend Mr. P. Taylor, of Bury court, St. Mary Axe, to melt some for me in a close vessel; and this plan succeeded perfectly. The vessel employed on this occasion, was a kind of

* From the Technical Repository, No. I.

copper

copper flask containing a horizontal stirrer or agitator, which being kept in motion by means of a handle rising above the flask, prevented the caoutchouc from burning to the bottom.

I am, dear sir, yours,

A. Aikin.

P. S.—In the preceding notice I have stated the method of applying the caoutchouc precisely as I have myself practised it, and as I communicated it to Mr. Perkins*. To him is owing the suggestion of incorporating the caoutchouc with oil of turpentine; which makes it more easy in its application; and has the further advantage of causing the caoutchouc to dry into a firm tough varnish, impenetrable to moisture, and capable at any time of being removed by means of a soft brush charged with warm oil of turpentine.

T. Gill, Esq.

* Mr. Perkins employs the caoutchouc in preserving his engraved steel blocks, plates, rolls, dies, &c. from oxidation.

LIX. *On the Eclipses of Jupiter's Satellites during the present Year*.*

This Table contains a list of all the Eclipses of Jupiter's satellites, marked as visible at Greenwich, deduced from the *Connaissance des Tems* for 1822, by deducting the difference of the meridians, or, 9′ 21″. The times of the eclipses, in that work, have been computed from M. Delambre's new tables published in 1817†. I have calculated several of them, and find them correct. I know not from what tables those in the Nautical Almanac have been computed (the laudable custom of informing the public on these points having been for some years omitted), but there is so striking a difference between the results in the two works, that I thought it might be acceptable to the practical astronomer to have them presented at one view. The differences amount, in some cases, to 2′ 10″. If the computations in the Nautical Almanac have been made (as formerly) by *two separate persons*, and should prove incorrect, it is singular they should both have fallen into precisely the same errors. The list contains only those eclipses which are recorded in both works. The last column may be useful to the observer when looking out for

* From Mr F. Baily's "Astronomical Tables and Remarks for the year 1822," a work printed for private circulation only.

† The Commissioners of the Board of Longitude have deferred the use of these tables till the year 1824, a period of seven years from the date of their publication. This is nearly fulfilling the injunction of Horace: *nonumque prematur in annum*. It certainly gives ample time for the detection of any errors.

an emersion*. It denotes the distance of the satellite from Jupiter's limb, at the moment of its re-appearance; the diameter of Jupiter being taken for unity. This distance is to be measured either in a line with Jupiter's equator (or longer axis), or in a line parallel thereto. Or, which is the same thing, in a line with the belts: for the satellites generally appear a little above or below the centre.

Before I dismiss this subject of the eclipses of Jupiter's satellites, I would call the attention of the practical astronomer to that of the *shadows of the satellites passing over the face of Jupiter*. On the importance of such observations M. Laplace has the following remark: "Les observations de l'entrée et de la sortie de leurs ombres sur le disque de Jupiter, répandraient beaucoup de lumière sur plusieurs élémens de cette théorie. Ce genre d'observations, *jusqu'ici trop négligé par les astronomes*, me paraît devoir fixer leur attention, car il me semble que les contacts intérieurs des ombres doivent déterminer l'instant de la conjonction, avec plus d'exactitude encore que les éclipses. La théorie des satellites est maintenant assez avancée pour que ce qui lui manque ne puisse être déterminé que par des observations très-précises. Il devient, donc, nécessaire d'essayer de *nouveaux moyens d'observation*, ou du moins, de s'assurer que ceux dont on fait usage, méritent la préférence †." I am not aware of any recorded observations of this nature: and a new and interesting field is thus opened to those practical astronomers who are fortunately possessed of powerful telescopes.

* "The telescopes, *proper* for observing the eclipses of Jupiter's satellites, are common refracting telescopes from *fifteen* to *twenty* feet." So says the Nautical Almanac: but I much doubt whether any one of the Commissioners of the Board of Longitude ever saw a telescope of this kind; nor do I think there is such a thing in existence. How absurd then it appears to *recommend* the use of them; and thus *mislead* (as I know it has done) those entering the career of science! A great part of the utility and importance of observations of these eclipses arises from the use of telescopes of nearly the *same* form and power: by which means the times of the phænomena are more readily *compared*. Telescopes with *three* object glasses are now rarely made: and those with *two* object glasses, of 46 inch focal length and 3¾ inches aperture, will perhaps, in the present state of the art, be found the *most proper* for observations of this kind. If the observation of occultations of the fixed stars by the moon should be introduced into the Navy, a much smaller telescope will answer for such purposes.

† *Système du monde*, page 252, 4th edition.

1822.		Satellite.		Mean time at Greenwich.			Diff. of Naut. Alm.		Distance from ♃
				h	′	″	′	″	
July	6	im.	—	15	18	46	+0	10	
	16	—	III	13	28	21	+1	50	
	16	em.	—	15	33	40	+1	6	1·16
	22	im.	I	13	34	54	+0	16	
	29	—	—	15	28	37	+0	16	
Aug.	5	—	II	13	21	23	−0	8	
	12	—	—	15	58	22	−0	6	
	14	im.	I	13	44	19	+0	13	
	21	em.	III	11	34	51	+0	57	1·36
	21	im.	I	15	37	51	+0	13	
	28	—	III	13	27	21	+1	46	
	28	em.	—	15	34	59	+0	57	1·37
	30	im.	I	14	59	43	+0	16	
Sept.	6	—	II	13	7	30	+0	1	
	6	—	I	13	53	15	+0	13	
	13	—	II	13	44	38	+0	4	
	13	—	I	15	46	46	+0	11	
	15	—	—	10	15	10	+0	9	
	22	—	—	12	8	39	+0	12	
	29	—	—	14	2	15	+0	9	
Oct.	1	—	II	10	18	11	+0	6	
	3	—	III	9	25	46	+1	11	
	3	em.	—	11	35	58	+0	19	1·16
	6	im.	I	15	55	52	+0	8	
	8	—	—	10	24	18	+0	5	
	8	—	II	12	55	19	+0	13	
	10	—	III	13	25	7	+1	22	
	10	em.	—	15	35	52	+0	53	1·05
	13	im.	I	17	49	34	+0	6	
	15	—	—	12	18	2	+0	3	
	15	—	II	15	32	34	+0	12	
	17	—	III	17	24	53	+1	42	
	22	—	I	14	11	50	+0	2	
	22	—	II	18	9	48	+0	11	
	24	—	I	8	40	18	+0	3	
	26	—	II	7	27	57	+0	11	
	29	—	I	16	5	45	[illegible]0	0	
	31	—	—	10	34	14	+0	3	
Nov.	2	[illegible]	II	10	5	14	+0	13	
	5	[illegible]	I	17	59	47	0	0	
	7	—	—	12	28	17	+0	3	

1822.		Satellite.		Mean time at Greenwich.			Diff. of Naut. Alm.		Distance from ♃.
				h	′	″	′	″	
Nov.	8	im.	III	5	24	57	+2	10	
	8	em.	—	7	38	15	+1	17	·41
	9	im.	I	6	56	51	−0	1	
	9	—	II	12	42	30	+0	13	
	14	—	I	14	22	32	−0	1	
	15	—	III	9	25	40	+1	34	
	15	em.	—	11	39	41	+0	42	·23
	16	im.	I	8	51	8	−0	5	
	16	—	II	15	19	45	+0	14	
	21	—	I	16	16	53	−0	4	
	22	—	III	13	25	31	+1	30	
	22	em.	—	15	40	14	+0	38	·05
	23	im.	I	10	45	32	−0	8	
	25	em.	—	7	22	3	−0	18	·01
	27	—	II	9	50	58	+0	14	·05
	29	im.	III	17	25	24	+1	32	
	30	em.	I	14	18	5	−0	22	·06
Dec.	2	—	—	9	16	45	−0	20	·08
	4	—	II	12	28	9	+0	15	·16
	7	—	I	16	42	54	−0	25	·13
	9	—	—	11	11	36	−0	23	·15
	11	—	—	5	40	22	−0	26	·17
	11	—	II	15	5	17	+0	17	·28
	15	—	—	4	23	38	+0	17	·34
	16	—	I	13	6	36	−0	24	·22
	18	—	—	7	35	23	−0	27	·24
	21	im.	III	5	27	31	+2	10	
	21	em.	—	7	45	12	+1	17	·68
	22	—	II	7	0	46	+0	16	·45
	23	—	I	15	1	44	−0	25	·29
	25	—	—	9	30	34	−0	29	·31
	28	im.	III	9	29	0	+1	28	
	28	em.	—	11	47	26	+0	40	·88
	29	—	II	9	37	51	+0	17	·54

LX. *On the Culture of the Pear Tree.* By Thomas Andrew Knight, *Esq. F.R.S. &c.**

The pear-tree exercises the patience of the planter during a longer period, before it affords fruit, than any other grafted tree which finds a place in our gardens; and though it is subsequently very long-lived, it generally, when trained to a wall, becomes in a few years unproductive of fruit, except at the extremities of its lateral branches. Both these defects are, however, I have good reason to believe, the result of improper management; for I have lately succeeded most perfectly in rendering my *old* trees very productive in every part, and my *young* trees have almost always afforded fruit the second year after being grafted, and none have remained barren beyond the third year.

In detailing the mode of pruning and culture I have adopted, I shall probably more easily render myself intelligible, by describing, accurately, the management of a single tree of each.

An *old* St. Germain pear-tree, of the spurious kind, had been trained, in the fan form, against a North-west wall in my garden, and the central branches, as usually happens in old trees thus trained, had long reached the top of the wall, and had become wholly unproductive. The other branches afforded but very little fruit, and that never acquiring maturity, was consequently of no value; so that it was necessary to change the variety, as well as to render the tree productive.

To attain these purposes, every branch which did not want at least twenty degrees of being perpendicular, was taken out at its base; and the spurs upon every other branch, which I intended to retain, were taken off closely with the saw and chisel. Into these branches, at their subdivisions, grafts were inserted at different distances from the root, and some so near the extremities of the branches, that the tree extended as widely in the autumn, after it was grafted, as it did in the preceding year. The grafts were also so disposed, that every part of the space the tree previously covered was equally well supplied with young wood.

As soon in the succeeding summer as the young shoots had attained sufficient length, they were trained almost perpendicularly downwards, between the larger branches and the wall to which they were nailed. The most perpendicular remaining branch upon each side was grafted about four feet below the top of the wall, which is twelve feet high; and the young shoots, which the grafts upon these afforded, were trained inwards, and bent down to occupy the space from which the old central branches had been taken away, and therefore very little vacant space any where remained in the end of the first autumn. A

* From the Transactions of the London Horticultural Society.

few

few blossoms, but not any fruit, were produced by several of the grafts in the succeeding spring; but in the following year, and subsequently, I have had abundant crops, equally dispersed over every part of the tree; and I have scarcely ever seen such an exuberance of blossom as this tree presents in the present spring (1813). Grafts of eight different kinds of pears had been inserted, and all afforded fruit, and almost in equal abundance. By this mode of training, the bearing-branches, being small and short, may be changed every three or four years, till the tree is a century old, without the loss of a single crop; and the central part, which is unproductive in every other mode of training, becomes the most fruitful. When a tree, thus trained, has perfectly covered the wall, it will have taken very nearly the form recommended by me in the Horticultural Transactions of 1808, except that the small branches necessarily pass down behind the large. I proceed to the management of young trees.

A young pear-stock, which had two lateral branches upon each side, and was about six feet high, was planted against a wall early in the spring of 1810; and it was grafted in each of its lateral branches, two of which sprang out of the stem about four feet from the ground, and the others at its summit, in the following year. The shoots these grafts produced, when about a foot long, were trained downwards, as in the preceding experiment, the undermost nearly perpendicularly, and the uppermost just below the horizontal line, placing them at such distances, that the leaves of one shoot did not at all shade those of another. In the next year, the same mode of training was continued; and in the following, that is the last year, I obtained an abundant crop of fruit, and the tree is again heavily loaded with blossoms.

This mode of training was first applied to the Aston Town Pear, which rarely produces fruit till six or seven years after the trees have been grafted; and from this variety, and the Colmar, I have not obtained fruit till the grafts have been three years old.

LXI. *Results of a Meteorological Journal for the Year* 1821, *kept at the Observatory of the Academy, Gosport. By* William Burney, *LL.D.*

Gosport, March 15, 1822.

Sir,—I herewith forward you the results of my last year's Meteorological Journal for the Philosophical Magazine, if you should deem them worth inserting. They are on a more extensive scale than the results of registers in general, and therefore will afford more information. I should have sent them early in February, had I not been unavoidably prevented; and am,

Sir, your very obedient servant,

To Dr. Tilloch. William Burney.

Results of a Meteorological Journal for the year 1821, kept at the Observatory of the Academy, Gosport.

Lat. 50° 47′ 38″ North. Long. 1° 6′ 40″ West of Greenwich. In Time, 4′ 26″·7.

Months.	Barometer. Max.	Barometer. Min.	Barometer. Media.	Barometer. Range.	Barometer. No of Changes.	Barometer. Spaces described.	Barometer. Greatest Variation in 24 hrs.	Barometer. Media at 8 A.M.	Barometer. Media at 2 P.M.	Barometer. Media at 8 P.M.	Self-registering Thermometer. Max.	Self-registering Thermometer. Min.	Self-registering Thermometer. Media.	Self-registering Thermometer. Range.	Self-registering Thermometer. Gt. Var. in 24 hrs.	Self-registering Thermometer. Media at 2 P.M.	Self-registering Thermometer. Media at 8 A.M.	Self-registering Thermometer. Media at 8 P.M.	Mean Temp. of Spring Water.	De Luc's Hygrometer. Max.	De Luc's Hygrometer. Min.	De Luc's Hygrometer. Mean Range of the Index.	De Luc's Hygrometer. Media at 2 P.M.	De Luc's Hygrometer. Media at 8 A.M.	De Luc's Hygrometer. Media at 8 P.M.	De Luc's Hygrometer. Media at 8. 2 & 8.
	In.	In.	In.	In.		In.	In.	In.	In.	In.	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
January ...	30·80	28·70	29·795	2·10	21	6·80	1·08	29·79	29·81	29·79	54	24	41·27	30	17	43·71	39·16	40·55	48·71	100	58	42	79·3	85·5	86·2	83·6
February	30·88	28·84	30·232	2·04	18	5·69	0·78	30·24	30·22	30·23	52	25	38·25	27	21	43·36	34·78	36·82	49·75	90	47	43	60·6	73·7	70·5	68·3
March ...	30·39	28·72	29·475	1·67	22	9·41	0·73	29·46	29·47	29·48	60	31	45·63	29	22	50·52	42·81	43·35	49·00	100	44	56	63·8	77·4	78·1	73·1
April	30·16	29·10	29·499	1·06	25	6·84	0·75	29·49	29·50	29·51	74	33	52·03	41	25	58·57	50·70	49·20	49·61	92	37	55	53·3	66 1	69·5	63·0
May	30·26	28·85	29·754	1·41	16	7·19	0·74	29·75	29·76	29·76	69	32	52 27	37	25	58·29	52·71	51·03	50·11	95	35	60	50·6	56·7	60·8	56·0
June	30·34	29·44	29·988	0·90	21	3·80	0·52	29·99	29·99	29·98	74	42	58·25	32	24	65·33	57·53	56·50	51·06	71	34	37	43·4	53·8	50·2	49·1
July	30·34	29·62	29·966	0·72	27	4·76	0·38	29·96	29·98	29·97	76	44	61·52	32	26	68·09	61·65	59·55	52·08	100	35	65	51·2	59·5	64·1	58·3
August ...	30 22	29·55	29·966	0·67	21	3·90	0·43	29·96	29·96	29·96	80	52	65·80	28	24	71·74	64 58	63·90	53·30	100	44	56	66·4	75·1	79·1	73·5
September	30 29	29·50	29·929	0·79	27	5·55	0·46	29·92	29·93	29 93	77	45	63 06	32	20	68·23	61·37	60·40	54·57	100	54	46	70·9	80·2	88·9	80 0
October ...	30·39	29·04	29·942	1·35	24	7·63	0·83	29·94	29·94	29·95	68	39	54·43	29	21	58·93	52·22	52·26	54·68	100	60	40	73·9	86·2	88·0	82·7
November	30·32	29·[illegible]	29·843	1·04	29	7·49	0·53	29·84	29·84	29·85	65	31	51·72	34	20	54 53	50·63	51·33	53·82	100	54	46	73·4	83·2	83·9	80·2
December	30·30	28·10	29·494	2·20	25	11·45	1·17	29·48	29·49	29·51	58	36	47·71	22	17	50·06	45 97	47·32	52·57	100	63	37	76·1	84·4	84·9	81·8
Averages for 1821.	30·88	28·10	29·823	15·95	276	80·51	1·17	29·81	29·82	29·82	80	24	52·66	31·1	26	57·61	51·17	51·01	51·60	100	34	48·6	63·5	73·4	75·3	70·8
Averages for 1820.	30·72	28·40	29·873	14·77	271	71·65	0·98	29·88	29·88	29·88	87	14	50 13	34·0	30	55·61	49·00	48·77	......	100	33	54·2	58·4	67·9	71.7	65 9

Months.	Scale of the Winds. North.	North-East.	East.	South-East.	South.	South-West.	West.	North-West.	Total Number of Days.
January...	½	7	6	4	3½	5	3	2	31
February	5	6	5½	3	2	2	2½	2	28
March	3	1½	1	4½	2	9	6	4	31
April	1	4	2	2	4	4½	7½	5	30
May	3½	3	3	3	1	5½	5	7	31
June	5	14		2		2	5	2	30
July	1	3	2	3	1	10	3	8	31
August ...		3	5	5	1	5	7	5	31
September	½	½		3	1	11	5	9	30
October...	2	1	3	4	3	9	4	5	31
November			1	5	3½	10	5½	5	30
December	1	½	½	5	4	10	7	3	31
Averages for 1821.	22½	43½	29	43½	26	83	60½	57	365
Averages for 1820.	35¾	38½	54	26	36	51	71¾	53	366

Months.	Modifications of Clouds. Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostratus.	Nimbus.	Weather. A clear Sky.	Fair, with Clouds.	An overcast Sky.	Fog.	Rain, &c.	Total Number of Days.
January...	11	8	18	...	5	20	18	1	7	10	4	½ 9	31
February	15	13	20	5	13	15	5	4	12	11	...	1	28
March	19	12	23	...	20	17	22	2	12	4	1	12	31
April	20	17	26	4	23	25	21	1	15	4	1	9	30
May	21	20	27	2	27	22	19	3	14	5	...	9	31
June	25	19	23	2	24	21	11	1	15	10	...	4	30
July	21	28	28	4	27	18	14	2	16	6	...	7	31
August ...	19	18	25	10	20	17	14	5	11	8	1	6	31
September	22	23	28	5	20	25	19	2	11	8	...	9	30
October...	17	14	26	8	19	18	15	4	13	6	½	7½	31
November	21	21	27	2	12	20	23	1	9	10	...	10	30
December	22	15	29	1	16	22	27	1	9	6	...	15	31
Averages for 1821.	233	208	300	43	226	210	208	27	141	88	7½	98½	365
Averages for 1820.	208	199	283	50	186	188	170	32	169½	76	5½	83	366

Months.	Atmospheric Phænomena. Anthelia.	Parhelia.	Paraselenæ.	Solar Halos.	Lunar Halos.	Rainbows.	Meteors.	Lightning.	Thunder.	Evaporation in Inches, &c.	Rain, &c. in Inches, &c.
January...	...	2	...	1	5	1	...	...	...	0·41	2·64
February	1	2	...	1	1	...	3	...	...	0·98	0·18
March	2	4	...	3	4	7	4	...	...	1·15	4·18
April	1	16	...	10	2	6	7	4	2	2·20	3·39
May	1	8	...	3	2	2	2	1	...	2·85	3·18
June	...	6	...	1	...	...	1	...	...	3·05	0·98
July	...	...	...	5	...	...	18	1	1	2·90	3·65
August ...	...	2	...	1	..	2	71	3	...	2 60	3·71
September	...	8	6	3	5	4	30	2	1	2·00	3·34
October...	...	8	...	...	3	5	21	..	...	1·40	4·53
November	1	1	1	5	4	2	15	2	1	1·35	6·02
December	...	1	2	2	8	6	17	5	2	0·97	7·61
Averages for 1821.	6	48	9	38	31	35	201	18	7	21·86	43·41
Averages for 1820.	1	34	10	39	24	14	131	14	6	29·35	25·75

ANNUAL

ANNUAL RESULTS FOR 1821.

Barometer.	Inches.
Greatest pressure of the atmosphere, Feb. 6th. Wind S.	30·88
Least do. of do. Dec. 24th. Do. S.E.	28·10
Range of the mercury	2·78
Annual mean pressure of the atmosphere	29·823
Mean pressure for 170 days with the Moon in North declination	29·905
Mean pressure for 183 days with the Moon in South declination	29·784
Annual mean pressure at 8 o'clock A.M.	29·818
———— at 2 o'clock P.M.	29·824
———— at 8 o'clock P.M.	29·826
Greatest range of the mercury, in December ..	2·200
Least range of do. in August ..	0·670
Greatest annual variation in 24 hours, in December	1·170
Least of the greatest variations in 24 hours, in July	0·380
Spaces described by the alternate rising and falling of the mercury	80·510
Number of changes caused by the variations in the weight of atmospheric column	276.

Self-registering Day and Night Thermometer.	
Greatest thermometrical heat, August 23d. Wind S.E.	80°
———— cold, January 2d. Wind E.	24
Range of the thermometer between the extremes ..	56
Annual mean temperature of the atmosphere ..	52·66
———— of do. at 8 A.M. ..	51·17
———— of do. at 8 P.M. ..	51·01
———— of do. at 2 P.M. ..	57·61
Greatest range in April	41·00
Least of the monthly ranges in December	22·00
Annual mean range	31·08
Greatest annual variation in 24 hours in July ..	26·00
Least of the greatest variations in 24 hours in January and December	17·00
Mean temperature of spring water at 8 A.M. ..	51·60

De Luc's *Whalebone Hygrometer.*	Degrees.
Greatest humidity of the atmosphere, 32 times ..	100
Greatest dryness of the atmosphere on the 29th of June	34
Range of the hygrometer between the extremes ..	66
Annual mean of do. at 8 A.M.	73·4
———— at 8 P.M.	75·3
———— at 2 P.M.	63·5
———— at 8, 2, and 8 o'clock	70·8

Greatest

Greatest monthly mean humidity of the atmosphere in January	83·6
Greatest monthly mean dryness of the atmosphere in June	49·1

Position of the Winds.	Days.
From North to North-East	22½
—— North-East to East	43½
—— East to South-East	29
—— South-East to South	43½
—— South to South-West	26
—— South-West to West	83
—— West to North-West	60½
—— North-West to North	57

Clouds, agreeably to the Nomenclature; or the Number of Days on which each Modification has prevailed.

	Days.
Cirrus	233
Cirro-cumulus	208
Cirro-stratus	300
Stratus ..	43
Cumulus ..	226
Cumulo-stratus	240
Nimbus ..	208

General State of the Weather.	Days.
A transparent atmosphere, without clouds ..	27
Fair, with various modifications of clouds ..	144
An overcast sky, without rain	88
Fog	7½
Rain, hail, sleet, and snow	98½
	——365

Atmospheric Phænomena.	No.
Anthelia, or mock-suns diametrically opposite to the true sun	6
Parhelia, or mock-suns	48
Paraselenæ, or mock-moons	9
Solar halos	38
Lunar halos	34
Rainbows, perfect	35
Meteors of various sizes	201
Aurora borealis in the night of the 25th of March	1
Lightning, days on which it occurred ..	18
Thunder do. do.	7

Evapo-

Evaporation.		Inches.
Greatest monthly quantity in June	..	3·05
Least monthly quantity in January	..	0·41
Total amount for the year	..	21·86

Rain, &c.		Inches.
Greatest monthly quantity in December	..	7·61
Least monthly quantity in February	..	0·18
Total amount for the year ..	..	43·41

N. B.—The barometer is hung up in the Observatory fifty feet above low-water mark; and the self-registering horizontal day and night thermometer, and De Luc's whalebone hygrometer, are placed in open-worked cases, in a northern aspect, out of the rays of the sun, ten feet above the garden ground. The pluviameter and evaporator have respectively the same square area: the former is emptied every morning at 8 A.M., after rain, into a cylindrical glass gauge accurately graduated to 1-100dth of an inch; and the quantity lost by evaporation from the latter, is ascertained at least every third day, and sometimes oftener, when great evaporations happen by means of a high temperature, and dry northerly or easterly winds.

Barometrical Pressure.—In the course of the year the mercurial column has met with an unprecedented range, having risen higher and sunk lower than we ever saw it before. Its greatest elevation occurred in February, which was characterized by fair and frosty weather, and was the coldest month in the year; and its greatest depression happened at midnight of the 24th of December, a remarkably wet and windy month. (See rain column in the table, and the London Magazine for February 1822 for the remarks made at the time.) The range between the annual extremes is 2·72 inches. The year having been wet, particularly the last four months, and the elasticity of the atmosphere much disturbed by prevailing gales of wind, the annual mean pressure, therefore, is also unprecedented, being 1-20th of an inch lower than that of last year, and rather more than 1-20th of an inch lower than the mean for the last seven years.

The aggregate of the spaces described by the mercury in its alternate rising and falling is 8·86 inches more than that of the preceding year, and the number of changes five more.

For 170 days of this year while the moon was in North declination, the mean pressure was 1-8th of an inch higher than that in the 183 days in which she ranged in South declination. Last year the mean pressure was greatest while she was in South declination, and *vice versâ* the year before.

Temperature.—The mean temperature of the air, considering its wet and windy state, and the decrease in the average, of May, June, and July, are strikingly great, being more than $2\frac{1}{2}°$ higher than that of last year, and equal to the warm year 1818 within 1-8th of a degree. This arises chiefly from the more uniform temperature of the days and nights during the last five months. Contrary to the course of the season, the mean temperature of February was three degrees lower than that of January, and the mean of April nearly equal to that of May. September was more than $1\frac{1}{2}°$ warmer than July; and November within 11-20ths of a degree of the mean of May. The mean temperatures at 8 A.M. and 8 P.M. without doors, coincide with each other within about 1-6th of a degree; but they deviate from the annual mean $2\frac{1}{2}°$, which is more than usual.

The mean temperature at 8 A.M. and 8 P.M. *within* doors, is $2\frac{1}{5}°$ higher than that *without* at the same hours, and only 3-5ths of a degree higher than the annual mean.

The annual mean temperature of spring water, as ascertained by about eight observations every month at 8 A.M., is rather more than one degree under the annual mean temperature of the air without doors. By these observations it appears that the ground did not arrive at its *maximum* heat till the autumnal equinox, which was one month after the *maximum* heat of the air; and that the greatest monthly mean temperature of spring water was in October. How far this will agree with the usual time of the greatest mean monthly heat of the ground, subsequent years' observations will determine, as we have no comparison to make by a reference to former years, but suspect that that was very late.

The mean state of the air by De Luc's whalebone hygrometer, is several degrees more towards the moisture point, than in the preceding years when a less quantity of rain fell.

Wind.—The wind has been very prevalent, and it blew longer from the S.W. this year than from any other point of the compass. From the preceding seven years' observations on the position of the wind, it appears that its longest duration is from the South-West. Its duration from the North this year, is about 2-3ds of the mean for the last seven years from that point. From S.E. and S.W. it has prevailed one-third more than the average of the former years, and from these points we had most of the late heavy rains. The winds from the East, South, and West points, have respectively fallen short of their average duration of former years; but those from N.W. and N.E. are nearly equal. By particular attention to the direct course of the modifications of clouds, we have this year been enabled to furnish additional proofs of the simultaneous existence of several currents

of

of wind, more especially at or near the changes from wet to dry, as pointed out in our *daily remarks on the weather in the London Magazine;* that the upper currents generally prevail over, and ultimately descend into the region of the lower ones; and that the wet or dry state of the weather here, very much depends on the position of the winds: those from the South-West seldom fail to bring up rain before they have subsided, perhaps from their crossing the Atlantic Ocean, where a greater abundance of vapours must undoubtedly exist by means of a more powerful evaporation, and be wafted hither by their influence, and which are condensed and precipitated in rain on arriving in colder regions over the soils of the land. The following is the number of strong gales, or days on which they have prevailed this year:

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
3	4	6	10	9	51	12	7	102

Hence it appears that the South-West and West winds are not only most prevalent in hard gales, but also in steady breezes and light airs, which is further corroborated by former years' observations.

Clouds.—All the modifications of clouds, or the days on which they have prevailed, appear by the table to be higher in numbers than in former years, except the *stratus*, which is nearly equal, or rather more than its average; yet this modification is generally a prognostic of fair weather.

It is natural that the *cirrus*, *cirrostratus*, and the compound *cumulostratus* (modifications that have a tendency by inosculation with others to produce rain), should exceed their average appearances of former years, as well as the *nimbus*, on account of the late heavy rains. The prevalence of the others shows that we have been favoured with intervals of fine weather, particularly as it respects the *cirrocumulus*, whose frequent appearance in great measure (it being an index to increasing heat near the earth's surface) accounts for the high annual mean temperature of the atmosphere. Under peculiar states of the atmosphere, we have recently seen this cloud evaporate while within 20° or 30° of the sun; and at other times we have seen it descend and transform itself into linear *cirrostratus*. The appearance of the *cumulus*, which is also a fair-weather cloud, has been more frequent by almost one-fourth of the average times of its appearance in former years.

Atmospheric Phænomena.—*Anthelia* have appeared oftener this year than in others. The great number of *parhelia* in April

April and May, and of *paraselenæ* in September, was remarkable. The number of solar and lunar *halos* is nearly equal; the greatest portion appeared in April and December, two wet months, a proof of their being prognostics of approaching rain, as is almost every other meteoric phænomenon. The frequent appearance of rainbows, both single and double, has enabled us to disprove Dr. Watt's new theory of their formation, as published by him in the Annals of Philosophy, vol. xiii. p. 131.

METEORS, both small and large, have also appeared frequently. Their connexion with, or appearance before, wind and rain, we have fully shown in the last volume of the Philosophical Magazine and Journal, from attentive and punctual observations. Besides these, we have observed other atmospheric phænomena, but not registered them, such as yellow lunar *coronæ* from 1° to 2° in diameter; lunar *discus halos*, and lunar *burrs*, which though inferior to others, prognosticate approaching wet; for at the time of their appearance a partial condensation of the atmosphere at a considerable height, and to a great extent, is not only evidently going on by means of additional vapours brought up by a current or currents of wind, but also frequently corroborated by the recession or sinking of the mercurial column, and a slight mist or haze near the horizon. By such observations as these, any one may determine for his own convenience the approach of rain some hours before its actual contact with the ground, without troubling himself about ascertaining the electrical state of the respirable air at the time. Should these prognostics fail at any time, which is seldom the case, it is caused by the combined influence of a superior wind, an increasing temperature, &c. that either dry up the descending vapours before their gravity is much augmented, or disperse them to some distant region. The appearance of the large solar and lunar *halos* determines the wet weather to be still nearer to us; and it is very rare that the vesicular vapours in which they are formed, are dispersed before their condensation and precipitation.

The EVAPORATION is less this year than in any of the preceding six years, on account of the frequent and heavy irrigations, and the low diurnal temperature of May, June, and July.

RAIN has fallen, more or less, on 208 days this year, of which 98 whole days and nights is the real time it has rained. From the 26th of August to the end of the year, there were only 37 dry days; of these a great portion were completely overcast and windy; and on the other 90 days, 23 inches of rain fell, which exceed the quantity for the preceding eight months. This certainly was the wettest period we have hitherto registered, and the distribution of the rain seems, from the various Meteorological Journals already published, to have been very unequal in different

different places, the greatest depth by far, after making every proper allowance for situation, being near the western coast.

This wet period having been attended with a mean temperature of 4°·61 higher than the mean of the same months (September, October, November and December) for the last seven years, it has therefore forwarded vegetation in a surprising manner. In the variable climate of Britain, scarcely a year passes but is productive of some anomalies in the state of the weather in one or other of the seasons; but the present year has produced many, as in the extremes of pressure, retrograde temperature, prevailing high winds, numerous atmospheric and meteoric phænomena, and rain exceeding in duration and quantity that of any former annual period.

LXII. *On the Distillation of Spirits from Grain, and on the Water most conducive to Fermentation.* By M. DUBRUNFAUT *of Lille**.

It is an opinion generally admitted in theory and in practice, that rain or river water is the most proper to produce a good fermentation. Those who have broached a different theory have contended that all sorts of waters, provided they are potable, are fit for the purpose. The first of these two opinions, although perhaps more unreasonable than the other, yet being founded on the greater purity which rain and river waters seem to the eye to possess, has prevailed for a long time unquestioned in many distilleries, where well or spring water would not be used without scruple.

This predilection, which I shall immediately show to be erroneous, has its origin in a false application of chemical theory. Indeed, when the delicate operations of analysis, and when the scrupulous manipulations of colours, require a water quite pure, and quite disengaged from every calcareous salt foreign to the results required, this may be readily conceived; but to extend this precaution to other operations of art, upon a simple probability and without examination, would be to fall into a similar error of prejudice to that which we have just been condemning.

The distillation of spirits from grain, which appears to have reached its greatest perfection in Germany, and particularly in Holland, is become now an important auxiliary to our agriculture, especially in the departments on the north and east sides of France.

French Flanders, which inherits in this branch of industry the long practice of the Dutch, possesses distilleries where they extract

* From *Annales de Chimie* for January 1822.

tract regularly 55–60, and even 65 litres of spirits at 19° from a quintal of barley. This statement may seem exaggerated to the distillers of the east and the interior, who do not obtain on an average more than from 40 to 44 litres from the same quantity of grain, and some scarcely from 30 to 35; but it is confirmed by the experience of a great many distilleries. Perhaps there is no art which presents anomalies more remarkable.

It would be curious to trace minutely the causes of these differences; but practice has got so much the advance of theory in this species of manufacture, that we are still forced to reason about it with much timidity. The fact which I am going to mention as explanatory of these differences, appears to me however sufficiently conclusive, and without pretending that it is the only cause, I believe it will be found at least the principal one.

Filled with chemical doctrines, I was surprised, on frequenting the premises of our distillers, to see them sinking at a great expense vast pits to procure water, when they might have supplied themselves cheaply from the river, which flowed close by. I asked them the cause of their preference; but without being able to explain it to me, they all agreed in answering that they still remembered too well the loss they had suffered from the employment of river-water ever to try it again. One person more observant whom I interrogated upon the quality of water best adapted for fermentation, answered, that it was that which flowed over rugged or unhewn fragments of stone.

I had here a ray of light. I recollected the means which Higgins had already pointed out to the planters of Jamaica, to prevent the acid fermentation, and I had no doubt that our well-water charged with carbonate of lime, held in solution with the aid of an excess of carbonic acid, might have the same effect on the processes of our distillers, as calcareous stones have less efficaciously on the fermenting processes of the Jamaica planters. In fact, this carbonate being dissolved, is desseminated equally through the whole vat, and it is thereby the readier to act on the molecules of the acid, which develop themselves so easily in a very dilute fermentation, and may prevent more completely the progress of that acetous fermentation so much dreaded by distillers.

I do not hesitate a moment in indicating this circumstance as an important cause of the great superiority of our distillers; and to this I am the more induced, since experience proves that they have never drawn more than from 40 to 44 litres, and often less, from a quintal of barley, where they have persisted in employing river-water for fermentation.

LXIII. *On*

LXIII. *On a Method of fixing a Transit Instrument exactly in the Meridian.* By F. Baily, *Esq. F.R.S. & L.S.**

The transit instrument is so essential a part of the apparatus of the practical astronomer, that every attempt to facilitate the use of it will doubtless be received with indulgence. When this instrument has been brought *nearly* in the plane of the meridian (which may be done by any of the methods pointed out in the several works on practical astronomy), it may be adjusted *accurately* by either of the following modes: 1°. by observation of the pole-star, at the time of its upper or lower culmination; 2°. by observing any of the circumpolar stars at the time of their upper and lower culmination; and 3°. by observing the culmination of any two stars differing from each other considerably in declination. The two former methods (independently of their requiring a building peculiarly constructed so as to command an uninterrupted view of the meridian, from the northern to the southern horizon) are liable to some objections, to which it is not my intention at present to advert: but the latter method may be practised in every situation in which a transit instrument may be placed, and as the results are extremely correct, I shall confine my remarks to this mode only of adjusting the instrument. Moreover, there are many persons, fond of practical astronomy, who have not the convenience, or who do not wish to incur the expense, of constructing a building of the kind above mentioned; and who are therefore compelled to fix their transit instruments on the sill of one of their windows, or in some other similar situation: many, again, who are travelling, with a view to improve the connected sciences of astronomy and geography, are obliged to fix their transit instruments in the most convenient and safe situation, where their prospect may be confined to a southern aspect:—to all such persons the method here alluded to, is the only one which can be adopted. Portable transit instruments, adapted to such purposes, are now made with great neatness and accuracy, and of various sizes; and are a valuable addition to every œconomical observatory, and to every person travelling for the purposes above mentioned. When placed on the *inner* sill of a window, they have a range of above 70° in altitude; and when placed on the *outer* sill, they may be pointed even to the zenith.

I shall therefore suppose that an instrument of this sort has been brought *nearly* in the plane of the meridian, by any of the known methods for that purpose: after which it may be *accurately* adjusted by determining its deviation from the meridian

* From the Memoirs of the Astronomical Society of London.

by the method, above mentioned, of observing the transit of two stars, differing considerably from each other in declination, and whose right ascensions are well ascertained. The principles of this method have been treated on by M. Lalande in his *Astronomie*, vol. ii. page 715; by M. Delambre in his *Astronomie*, vol. i. page 421; and by M. Biot in his *Traité d'Astronomie*, vol. iii. *Additions*, p. 130: with one or other of which I shall presume the reader to be previously acquainted.

The stars which should be chosen for the purpose, are those which differ at least 50 degrees from each other in declination: but the nearer that difference approaches to 90 degrees, the more correct will be the results. Their right ascensions, on the contrary, must be as near as possible to each other; a circumstance which will moreover prevent the possibility of any error arising from a variation in the rate of the clock during the interval of the observations. And here it may be proper to remark that the time, used in these computations, is *sidereal* time: if therefore a clock or watch, which marks *solar* time, be made use of, it must be corrected in the manner hereafter mentioned.

This being premised, it will be readily seen that, in this parallel of latitude, one of the stars will have *north* declination, and the other *south* declination: and, in order to avoid repetition, I shall call the former the *northern* star, and the latter the *southern* star. Their declinations I shall denote by N and S respectively: and it may be useful to know that they may be taken out to the nearest *minute* only, as great accuracy is not required in this respect. The right ascensions, however, of the two stars (which must be expressed in *time*) should be taken out from the most approved tables, and corrected for aberration and nutation*; in order that their apparent positions in right ascension may be exactly stated: on which indeed the accuracy of the method depends. The apparent right ascension of the northern star I shall denote by $Æ^n$, and the time of its observed passage, as shown by the clock, I shall denote by T^n: the apparent right ascension of the southern star I shall denote by $Æ^s$, and the time of its observed passage by T^s. The latitude of the place I shall denote by L; and the quantity sought (or the deviation of the instrument in azimuth) by A.

Now, in order to determine A, we must first take the difference of the apparent right ascensions of the two stars, and also the difference of the time of their observed transits; that is, we

* When the two stars are at *equal* distances from the equator, and differing but little from each other in right ascension, their *mean* places on the given day may be taken; as they will be nearly equally affected by aberration and nutation. Many *pairs* of stars, situated in this manner, may be mentioned: such as *β Geminorum* and *τ Navis*, *ν Coronæ Borealis* and *α Regulæ*, &c.

must

must make $(\text{Æ}^n - \text{Æ}^s) = d\text{Æ}$ and $(T^n - T^s) = dT$; and the formula for finding A will, agreeably to the principles laid down by MM. Delambre and Biot, be

$$A = (dT - d\text{Æ}) \times \frac{\cos N . \cos S}{\sin (N+S) \cos L}.$$

If the quantity $(dT - d\text{Æ})$ be *positive*, the deviation of the transit instrument will be to the *east* of the meridian: on the contrary, if it be *negative*, the deviation will be to the *west*. When it is $= 0$, the instrument is exactly in the plane of the meridian, and consequently does not require any correction.

By the help of a table expressing, for any given latitude, the value of $\frac{\cos N . \cos S}{\sin (N+S) \cos L}$ in numbers, according to the sum of the declinations (or the difference of the polar distances) of the two stars observed, we may, almost by inspection, obtain, in every case, the value of A, or the deviation of the transit instrument required; and consequently bring it afterwards exactly in the meridian, so as to be enabled to adjust it at any time to a meridian mark. The table, which I have here given, is calculated for the latitude of Greenwich ($= 51°. 28'. 40''$): but since it is not necessary to be very exact in the declination of the star, it will suit any other place not very distant from that parallel of latitude. I might have constructed the table so as to have been *general*, for all latitudes, by merely taking the value of $\frac{\cos N . \cos S}{\sin (N+S)}$; which value must then have been divided by the cosine of the latitude of the place where the observer might be situated. But, I have preferred, in the present instance, confining the table to the latitude of Greenwich; subjoining, however, a correction for the use of it in any other part of England.

The first perpendicular column of the table denotes the sum of the declinations (or the difference of the polar distances) of the two stars, for every degree from 42° to 72°: and opposite thereto is set down, in separate columns, the value for finding the deviation of the instrument in azimuth, according to the value of N, or the northern star, from 24° to 40°; those limits being sufficient for the purposes alluded to in the preceding part of this paper. The proportional part for any intermediate difference may be readily seen, on inspection. These values, multiplied by $(dT - d\text{Æ})$ or the difference between *the difference of the apparent right ascensions of the two stars, and the difference of their observed transits*, will show the value of A, or the total deviation of the instrument in *time*; which, multiplied by 15, will give the deviation in *arc*: and when the deviation of the instrument has been thus determined, it may be corrected in the usual manner. An example or two will best explain

explain the use and application of the table, and the mode of operating in such cases.

On July 1, 1819, I placed my transit instrument nearly in the meridian; and in order to ascertain how much it deviated from the true meridian I observed the two stars γ *Lyræ* and τ *Sagittarii*. The passage of the former was observed at 18ʰ. 52′. 37″,3, and of the latter at 18ʰ. 56′. 4″,5 sidereal time. The apparent right ascensions of those stars, on that day, were 18ʰ. 52′. 9″,8 and 18ʰ. 55′. 39″,7 respectively: and their declinations were 32°. 27′ north, and 27°. 55′ south. Consequently the operation will stand thus

$$\begin{array}{ll} \text{Æ}^n = 18^h.\ 52'.\ \ 9'',8 & T^n = 18^h.\ 52'.37'',3 \\ \text{Æ}^s = 18.\ \ 55.\ \ 39,7 & T^s = 18.\ \ 56.\ \ 9,9 \\ \hline d\text{Æ} = -\ \ \ 3.\ \ 29,9 & dT = -\ \ \ 3.\ 32,6 \end{array}$$

whence $(dT - d\text{Æ}) = -2'',7$. This value, being negative, shows that the deviation is to the west: and in order to determine the quantity of the deviation, we must take the sum of the declinations (or the difference of the polar distances) of the two stars; which in this case is equal to 60°. 22′; or, for the sake of round numbers, equal to 60°: and the declination of N (or the northern star) is about 32°. Consequently against the number 60 and under the column headed 32° we shall find 1·39; which being multiplied by $-2'',7$ will give $-3'',75$ for the deviation of the instrument in *time*: and this multiplied by 15 will give $-56'',3$ for the deviation in *arc* westerly.

Again, on Jan. 1, 1820, having reason to suspect that the transit instrument (from some motion which had been given to it) deviated from the plane of the meridian, I observed the passage of ϵ *Canis majoris*, and of *Castor*: the former at 6ʰ. 52′. 45″,6, and the latter at 7ʰ. 24′. 28″,4. The apparent right ascension of those stars on that day was 6ʰ. 51′. 34″,3 and 7ʰ. 23′. 7″,3 respectively; and their declinations were 28°. 44′ south and 32°. 16′ north. Consequently the operation will stand thus

$$\begin{array}{ll} \text{Æ}^n = 7^h.\ 23'.\ \ 7'',3 & T^n = 7^h.\ 24'.\ 28'',4 \\ \text{Æ}^s = 6.\ \ 51.\ \ 34,3 & T^s = 6.\ \ 52.\ \ 45,6 \\ \hline d\text{Æ} = +\ \ 31.\ \ 33,0 & dT = +\ \ 31.\ \ 42,8 \end{array}$$

whence $(dT - d\text{Æ}) = +9'',8$. The sum of the declinations (or difference of polar distances) being in this case 61°, we shall find that the value to be adopted is 1·36; which being multiplied by $+9'',8$ will give $+13'',33$ for the value of A in *time*, or (multiplying this by 15) $+3'.\ 20''$ for the value of A in *arc*. And this quantity being positive shows that the deviation was to the east.

If

If observations of this kind be made about sunrise or sunset, and after the passage of the stars, the telescope be pointed to the horizon and compared with some object there, a meridian mark may be set up, which may be corrected from time to time by subsequent observations on various stars similarly situated.

I have already stated that in all cases of this kind, the time employed is supposed to be sidereal time, and that if a clock or watch be used which marks mean solar time, the interval between the observations must be corrected accordingly. This correction is made by converting the value of dT (which is expressed in sidereal time) into mean solar time, in the usual manner, by adding the acceleration of the fixed stars for that interval. Thus, in the case last stated, suppose that the passage of ε *Canis majoris* had been observed at $12^h. 10'. 31'',6$, and the passage of *Castor* at $12^h. 42'. 9'',2$ mean solar time: the difference between these two (or dT) would be $31'. 37'',6$, to which the acceleration of the fixed stars for that interval $(=5'',2)$ must be added; whence the difference will be, as before, $=31'. 42'',8$. So that, by means of this correction, it will be indifferent whether the clock shows sidereal or mean solar time.

Before I close this paper I shall point out another important use to which these observations may be applied; namely to correcting the error of the clock at the time of observation. For after the quantity of the deviation is found, as above explained, the error of the clock may be determined by means of the transit of either of the stars employed; that is, of either N or S; but, for the sake of uniformity in the investigations, I shall confine my remarks to N, or the northern star. Let the observed time of the passage of N be denoted as before by T^n; and the apparent right ascension of N by $Æ^n$, and let the error of the clock at the time of observation be denoted by E. Then, from the principles laid down by M. Biot, we shall have

$$E = (T^n - Æ^n) + A . \frac{\sin(L-N)}{\cos N}.$$

The value of $\frac{\sin(L-N)}{\cos N}$ for the latitude of Greenwich I have thrown into numbers, and placed in the last line of the table at the end of this paper, so as to be ready for immediate use when required. It is denoted by c, since it serves to denote the correction of the clock. The application of the formula is very simple: the rule being as follows. From the observed time of the transit of the northern star deduct the apparent right ascension of the same star; to the difference add the product Ac: the sum is the error of the clock; which, when it is negative, shows that the clock is too slow.

For example; in the first case mentioned in this paper, the difference

difference between the observed and apparent time of the transit of γ *Lyræ* is $(18^h.\ 52'.\ 37'',3 - 18^h.\ 52'.\ 9'',8) = +27'',5$: the deviation of the transit instrument has been found to be $-3'',75$ in time, and the number in the table, against N ($=32°$) is ·39: the product of these two is $-1'',5$: so that $27'',5 - 1'',5 = 26'',0$ is the error of the clock at the time of observation, which being positive shows that the clock was too fast. I shall here repeat that the observed *time*, here alluded to, is supposed to be *sidereal* time: and therefore if *mean solar* time be employed in the observation, it must be converted into sidereal time, by any of the methods laid down for that purpose. It may be useful to remark that, in all observations of this kind, it is presumed that the proper adjustments of the transit instrument are made previously to observation: and particularly that the axis of the telescope is rendered perfectly level: otherwise the observation will partake of the error arising from this source, and render a further correction necessary.

I shall conclude by observing that M. Delambre prefers this mode of adjusting a transit instrument to that of observing the passage of the circumpolar stars, which requires an interval of at least 12 hours, during which time considerable alteration may have taken place in the rate of the clock; and therefore cannot be conveniently practised except when the days are very short, and in a building constructed peculiarly for meridional observations. Whereas the observations, here alluded to, may frequently be completed in a few minutes; at all times of the year; and often by daylight. The tables are very easily computed, and therefore every practical astronomer who requires greater accuracy should calculate them for the latitude of his own observatory. In which case, the labour will be very considerably abridged if he confines the table to the declination of those stars which are most frequently used by him for such comparisons.

It may be proper to state, that the values in this table (except those in the last line) must be multiplied by the following numbers, for any other parallel of latitude to the southward or northworth of Greenwich: viz. if

south 1°, by .979	north 3°, by 1.072
north 1°, by 1.023	—— 4°, by 1.099
—— 2°, by 1.047	

so that in no part of England will the correction amount to $\frac{1}{10}$th, nor if within 2 degrees of the latitude of Greenwich, will it amount to $\frac{1}{20}$th of the whole value. The last line, for the correction of the clock, is adapted to the latitude of Greenwich *only*.

Sum

Sum of Dec.	*Declination of the Northern Star N.*																
	24°	25°	26°	27°	28°	29°	30°	31°	32°	33°	34°	35°	36°	37°	38°	39°	40°
42	2.08	2.08	2.07	2.06	2.06	2.04	2.03	2.02	2.00	1.99	1.97	1.95	1.93	1.91	1.89	1.86	1.84
43	2.03	2.03	2.02	2.02	2.01	2.00	1.99	1.97	1.96	1.94	1.93	1.91	1.89	1.87	1.85	1.82	1.80
44	1.98	1.98	1.98	1.97	1.96	1.95	1.94	1.93	1.92	1.90	1.89	1.87	1.85	1.83	1.81	1.79	1.77
45	1.94	1.93	1.93	1.92	1.92	1.91	1.90	1.89	1.88	1.86	1.85	1.83	1.81	1.79	1.78	1.75	1.73
46	1.89	1.89	1.88	1.88	1.87	1.87	1.86	1.85	1.84	1.82	1.81	1.79	1.78	1.76	1.74	1.72	1.70
47	1.85	1.84	1.84	1.84	1.83	1.83	1.82	1.81	1.80	1.79	1.77	1.76	1.74	1.73	1.71	1.69	1.67
48	1.80	1.80	1.80	1.80	1.79	1.79	1.78	1.77	1.76	1.75	1.74	1.72	1.71	1.69	1.68	1.66	1.64
49	1.76	1.76	1.76	1.76	1.75	1.75	1.74	1.73	1.73	1.71	1.70	1.69	1.68	1.66	1.65	1.63	1.61
50	1.72	1.72	1.72	1.72	1.72	1.71	1.71	1.70	1.69	1.68	1.67	1.66	1.64	1.63	1.62	1.60	1.58
51	1.68	1.68	1.68	1.68	1.68	1.67	1.67	1.66	1.66	1.65	1.64	1.63	1.61	1.60	1.59	1.57	1.55
52	1.64	1.64	1.65	1.64	1.64	1.64	1.64	1.63	1.62	1.61	1.61	1.59	1.58	1.57	1.56	1.54	1.53
53	1.61	1.61	1.61	1.61	1.61	1.61	1.60	1.60	1.59	1.58	1.58	1.56	1.56	1.54	1.53	1.51	1.50
54	1.57	1.57	1.57	1.58	1.57	1.57	1.57	1.56	1.56	1.55	1.55	1.54	1.53	1.51	1.50	1.49	1.47
55	1.53	1.54	1.54	1.54	1.54	1.54	1.54	1.53	1.53	1.52	1.52	1.51	1.50	1.49	1.48	1.46	1.45
56	1.50	1.50	1.51	1.51	1.51	1.51	1.51	1.50	1.50	1.49	1.49	1.48	1.47	1.46	1.45	1.44	1.43
57	1.47	1.47	1.48	1.48	1.48	1.48	1.48	1.47	1.47	1.47	1.46	1.45	1.45	1.44	1.43	1.41	1.40
58	1.43	1.44	1.44	1.44	1.45	1.45	1.45	1.44	1.44	1.44	1.43	1.43	1.42	1.41	1.40	1.39	1.38
59	1.40	1.41	1.41	1.41	1.42	1.42	1.42	1.42	1.41	1.41	1.41	1.40	1.39	1.39	1.38	1.37	1.36
60	1.37	1.37	1.38	1.38	1.39	1.39	1.39	1.39	1.39	1.38	1.38	1.37	1.37	1.36	1.35	1.34	1.33
61	1.34	1.34	1.35	1.35	1.36	1.36	1.36	1.36	1.36	1.36	1.36	1.35	1.35	1.34	1.33	1.32	1.31
62	1.31	1.31	1.32	1.33	1.33	1.33	1.34	1.34	1.34	1.33	1.33	1.33	1.32	1.31	1.31	1.30	1.29
63		1.29	1.29	1.30	1.30	1.31	1.31	1.31	1.31	1.31	1.31	1.30	1.30	1.29	1.29	1.28	1.27
64			1.26	1.27	1.28	1.28	1.28	1.28	1.28	1.28	1.28	1.28	1.28	1.27	1.26	1.26	1.25
65				1.24	1.25	1.25	1.26	1.26	1.26	1.26	1.26	1.26	1.25	1.25	1.24	1.24	1.23
66					1.22	1.23	1.23	1.23	1.24	1.24	1.24	1.23	1.23	1.23	1.22	1.22	1.21
67						1.20	1.21	1.21	1.21	1.21	1.21	1.21	1.21	1.21	1.20	1.20	1.19
68							1.18	1.18	1.19	1.19	1.19	1.19	1.19	1.18	1.18	1.18	1.17
69								1.16	1.16	1.17	1.17	1.17	1.17	1.16	1.16	1.16	1.15
70									1.14	1.14	1.15	1.15	1.15	1.14	1.14	1.14	1.13
71										1.12	1.12	1.12	1.12	1.12	1.12	1.12	1.11
72											1.10	1.10	1.10	1.10	1.10	1.10	1.10
c =	·50	·49	·48	·46	·45	·44	·42	·41	·39	·38	·36	·35	·33	·31	·30	·28	·26

LXIV. *On the Cure of a Case of Paralysis by Lightning*.*

SINCE the period (1744) when Kratzenstein attempted for the first time to make electricity of service in the cure of several diseases, a great many works on this subject have been published. Some of them have announced cures almost miraculous; paralysis, tetanus, deafness, and various sorts of blindness, have all yielded to the application of this stimulant. Others have maintained, on the contrary, that electricity does not produce any useful effect. Perhaps it would be well in this state of incertitude to submit the question to a new examination. The contrariety which has been remarked in the results obtained by different phy-

* From the *Annales de Chimie for* 1822.

sicians

sicians equally worthy of credit, may be owing in a great measure to different manners of operating; some in fact contenting themselves with isolating the patient, and placing him in communication with the conductor of the machine, while others have regularly introduced the fluid into the suffering part by means of discharges more or less violent. But without saying more on this point at present, let us attend to the following *fact*, which we extract from one of the scientific journals published in America.

M. Samuel Leffers, of Carteret County, in North Carolina, had been seized with a paralytic affection which fixed itself on the face, and principally on the eyes. As he was walking in his chamber, a flash of lightning struck him down senseless; he came to himself at the end of twenty minutes, but he did not recover perfectly the use of his legs for the rest of the day and night. The next day he found himself quite recovered, and he sat down to write to one of his friends an account of what had happened to him; his letter was very long, and he wrote it without the help of glasses. Since then his paralysis has never returned. M. Leffers thinks that the same shock which restored his sight, has on the other hand injured the delicacy of his hearing.

The article from which we have extracted this case, is from the pen of M. Olmsted, professor of chemistry in the college of North Carolina.

LXV. *On Matting made from the* Typha latifolia, *or* Greater Cat's-Tail. *By Mr.* William Salisbury*.

The praiseworthy and successful endeavours of Mr. Salisbury, to open a new source of industry, peculiarly within the reach of the labouring poor, and of parochial workhouses, have received the approbation of the Society; both on their own account, and in the hope, that, by being recorded in their volume, they may excite others to similar exertions. A material hitherto unemployed, the spontaneous produce of pools and irreclaimable swamps in every part of the kingdom, peculiarly fitted to serve as the basis of domestic manufacture in the cottages of the poor, and the produce of which, whether sold or employed by the makers, will contribute essentially to the increase of their comforts, is not to be lightly passed over. One of the most serious privations to which cottagers in the agricultural districts are exposed, is that of cold during winter, arising in part from the inadequate shelter afforded by the hovels in which they live, and from the

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, p. viii. and p. 52. The Society's Ceres Medal was voted to Mr. Salisbury for this communication.

want

want of bedding. Their own pecuniary resources are but too often insufficient to supply the more imperious demands for food and clothing; so that, in ordinary circumstances, their sufferings from cold, during the hours intended by nature for repose and restoration, are excessively severe; as those well know, who have seen, with satisfaction not unmingled with sorrow, the joy which the donation of a single blanket invariably produces. If those who have the opportunity, would instruct and encourage the industrious poor in the manufacture of matting from the *Typha*, they would thus be enabled to supply themselves with an article, which, when employed as a cover to their damp floors, as curtains to their couches, and as an auxiliary to their scanty stock of bedding, would most materially contribute both to their comfort and to their health.

The material of which matting, and the rush-bottoms (as they are called) of chairs, are usually made, is the *Scirpus lacustris*, known in some parts of England by the name *bull-rush*, and in Durham and Northumberland by that of *pelecive*. It grows naturally in deep slow streams, and is particularly abundant in the neighbourhood of Newport Pagnel in Buckinghamshire.

The demand for this article, however, in the Newport Pagnel manufactories is considerably greater than that district can supply; and, in consequence, large importations of the *Scirpus* are made from Holland. Hence, in time of war, the article is often scarce, and at an exorbitant price.

Prior to the winter of 1817, Mr. Salisbury, induced by a laudable desire of opening new sources of industry to the unemployed poor, attempted, in various ways, to apply the leaves of the *Typha latifolia* (*flag*, or greater *cat's-tail*) to the same purposes as the *Scirpus*. For this purpose he was allowed, by the overseers of the parish of St. George, Hanover-square, to employ some of their paupers in collecting about 2½ tons of the *Typha* from the marshy grounds about Little Chelsea and Clapham; and afterwards in manufacturing a part of it into mats, baskets, hassocks, chair-bottoms, &c.

Samples of these various articles were laid before the Society in December 1817; and it appeared, that with equal skill in manipulation, equally neat work might be produced from the *Scirpus* and from the *Typha*. It being, however, a matter of considerable importance to ascertain the relative durability of the two articles under similar circumstances of ordinary wear, the following experiment was made:—A piece of the best Dutch matting, at 2*s*. 6*d*. a yard, and a similar one of Mr. Salisbury's manufacture, were laid down side by side in the Society's premises on the 13th of December 1817. Their relative situations were occasionally changed, in order to equalize, as nearly as pos-

 sible,

sible, the wear to which they are exposed; and on 27th March 1821 they were taken up and examined by the Committee of Manufactures. On minute inspection, they appeared to be about half worn out, and there was no very perceptible difference in the condition of each.

With regard to the relative expense of procuring and preparing the two articles for manufacture, the Society possess no very certain data; as the use of the *Typha* was at first set on foot chiefly in order to employ those parish poor who would otherwise have been idle. Two guineas were paid by Mr. S. for liberty to cut as much of the *Typha* as he pleased from about ten acres of swampy land near Hammersmith. The matting has been sold at from 9*d.* to 15*d.* per yard, and between 1000 and 1500 yards have been disposed of during the last three years.

The *Typha* abounds in all marsh ditches and uncultivated swampy ground in every part of the kingdom; whereas the *Scirpus* is found in quantity sufficient for manufacture only in certain districts: hence the former must be much more accessible and cheaper than even the *Scirpus* of home growth; and the Society indulge the hope, that, by giving this notice a place in their annual volume, the knowledge and the use of so abundant and cheap a material may be extended throughout the kingdom, and may form a means of domestic employment to the younger members of poor families.

LXVI. *On a luminous Appearance seen on the dark Part of the Moon in May* 1821. *Communicated in a Letter to the Rev. Dr.* Pearson, *from the Rev.* M. Ward *.

Dear Sir,—I have this moment laid aside my telescope from an examination of the moon. * The atmosphere was more favourable for the purpose than I have observed it to be for many weeks; and as it so happened, that at about the same age of the last moon, I had carefully examined the part in obscurity to look for a volcano, and had not in any part observed a remarkable appearance, I was greatly surprised to find a paragraph in the public papers, giving a detailed account of a volcano near *Aristarchus*, seen on the very night I had satisfied myself that there was not even an appearance which could be mistaken for a volcano. I resumed the attempt this evening; and having passed the enlightened part of the moon from the field, and carefully avoided looking at it, to have my eye in the best state to discover any more conspicuously illuminated spot in the unenlightened part, I soon saw *Aristarchus* very clearly, having very

* From the Memoirs of the Astronomical Society of London.

much

much the appearance of a small comet, on the moon's surface*. It was then half-past nine: the moon 15° high, and 40° 16′ west of the sun, I could perceive the shape to be extended towards *Grimaldus*, appearing in diameter equal to one of Jupiter's moons. I continued observing it till the moon was about 11° only high; when it extended itself to right and left horizontally, and became so very faint for the last degree as to be scarcely distinguishable: and having observed the occultation of a fixed star very near to it, at three minutes before ten I discontinued all further attention to it. The star at the instant before its occultation, from the then state of the atmosphere, appeared of about equal magnitude to, but far better defined than *Aristarchus* did at its most perfect appearance. My telescope magnifies about 80 times. The star which was occulted was 136 *Tauri:* and came in contact with the limb of the moon, as nearly as I could ascertain, at the advancing pole of libration; and the instant of occultation was at 10^h 5′ 55″,9 P.M., Greenwich time, estimating Tamworth 6′ 40″,8 in time west of Greenwich.

I had written thus far, when I recollected that, as the following day was not a post-day, I could not call your attention to it, and that I should lose nothing in point of time by observing the moon on the Saturday night; but it proved cloudy†.

Sunday night, a quarter before ten.—I have again examined

* Would it not be possible for the makers of telescopic eye-pieces to introduce a half-inch mother-of pearl micrometer (such as are usually divided into 100 equal parts) across the focus and field of the eye-glass, when the planets are the objects under examination? This would answer two valuable purposes. An observer might arrange that the planet should traverse the field entirely within the mother-of-pearl, and thus be enabled to prepare his eye by keeping it in darkness: perhaps he might thus observe a satellite of Saturn which he had never before seen; or by using this method with Venus (whose light is far too brilliant to allow a satellite to be seen), a more certain opinion would be obtained on the subject of her having or not having one. It may be applied even to the light which Mars diffuses over the field. But this method of viewing the planets is, I am aware, in direct contradiction to an assertion I have lately heard, that a very faint light is rendered visible by being near to, and perhaps within the diffusion of a superior one. I have not seen the arguments by which this opinion is supported, or I should not perhaps have suggested this mode of searching for satellites. The other use of the micrometer alluded to is that, as Saturn moves five seconds in an hour, the micrometer would measure any separation of a planet from every star supposed to be a satellite; and thus, after a few hours' motion of Saturn, put the inquiry beyond doubt.—M. W.

† *Note by one of the Secretaries.*—On the night here alluded to, when this phænomenon was invisible at Tamworth, on account of the clouds, it was distinctly seen by me in the neighbourhood of London, through a 3½ feet refracting telescope. Its appearance was nearly similar to that described by Mr. Ward.—F. Baily.

 the

the appearance, and find it about as distinct as it was about ten minutes before I discontinued observing on the night of the 4th. The spot is certainly Aristarchus; but it is now much more difficult to observe on account of the moon throwing much more light down the tube of the telescope, and the luminous advancing edge being much nearer the spot, and my telescope having a large aperture: but I should imagine, if my 42-inch tube were inclosed in one which projected five or six feet beyond the object-glass, that the spot might be seen one night at least longer. When I first examined on the 4th the proportion of light thrown on the moon by the earth, and consequently on Aristarchus, was 1·777 out of 2000; to-night it was only 1·422, a diminution of ·355; consequently exactly one-fifth less light is reflected by the spot, to say nothing of the inconvenience arising from the addition of one-fifth to the light of the moon. Hevelius describes Aristarchus under the name *Mons Porphyrites*, as *aut ex rupe rubrâ, aut sabulo* (this, by the by, is impossible; for the moon's attraction of gravity to its centre would not admit of a cavity of sand (loose sand) similar to Aristarchus) *sive terrâ rubicundâ constare, aut prorsus ardere, sive perpetuo igne exundare.* Its colour must therefore have greatly changed since 1644, for it is singularly *white* when illustrated by the sun; and when the other parts of the moon are yellow, or faintly red, this preserves its predominant whiteness; and its appearance on the 4th and 6th instants was similar to the light of the glow-worm. Could any light, such as we read is occasionally seen on the mountains of Asia Minor, or the phosphoric fire near Derbend, be peculiar to this cavity of the moon? and if so, has it changed, and does it change the colour of its flame?

Your polite attention to me when in town has occasioned my taking the liberty of troubling you with these hasty observations, which I would have put into a more regular form, but I am going to the philosophical lectures at Birmingham this evening; and, in order to save this day's post, I must now conclude with begging you to accept my esteem and thanks.

I am, dear sir,

Yours very sincerely,

Tamworth, May 4, 1821. MICHAEL WARD.

LXVII. *No-*

LXVII. *Notices respecting New Books.*

A Geological Survey of the Yorkshire Coast; describing the Strata and Fossils occurring between the Humber and the Tees, from the German Ocean to the Plain of York. Illustrated with numerous Engravings. By the REV. GEORGE YOUNG, *A.M. and* JOHN BIRD, *Artist.* 4to, pp. 328. Whitby, 1822.

It is not a little remarkable, that while philosophers have for ages been employed in contemplating those bright orbs which bespangle the sky, soaring on the wings of science through the regions of immeasurable space, surveying the magnitudes, stations and motions of the heavenly bodies, and in studying the laws which govern the remotest planets, little attention has been devoted to the planet we inhabit. It is true, that viewing the features and exploring the depths of the earth on which we tread, is not so attractive as the pursuits of astronomy; but the study is neither uninteresting nor unimportant, and has an equal claim on our attention.

That a study of such importance as geology should hitherto have had so small a share among scientific pursuits, is the more remarkable, since at a very remote period men began to penetrate into the bowels of the earth in quest of the shining metals, and other valuable products of the mineral kingdom; and the attention of the learned, both in ancient and modern times, has often been directed to the nature and classification of minerals. To these objects their pursuits were limited until within the last twenty or thirty years, few philosophers attempting to investigate the structure of the earth itself; or, if they did, they rather indulged in wild conjectures than entered into a sober and patient examination of the facts. Geology has now, however, begun to assume its proper rank among the sciences, and, desisting in a great measure from the flights of fancy, has been proceeding in the more legitimate track of laborious research. But although the collection of geological facts has been rapidly accumulating, yet, if we may judge from the jarring opinions held on the subject, we have not obtained sufficient data for establishing a general theory of the earth, or satisfactorily explaining the natural causes employed by the Creator to bring our globe into its present state; which, as all agree, is widely different from its original. Much has however already been achieved by the labours of geologists. They have examined the character and form of large and interesting portions of the crust of the earth in various regions of Europe, and particularly in the British Isles; and from a review of existing facts, they have arrived at some important conclusions, now generally admitted. The chief thing therefore to

to be done in the present stage of the science is to enrich it with ample stores derived from actual observation; to collect information concerning the character and relative positions of the substances composing the solid part of the globe; to specify their arrangement, extent and localities, and to notice such hints as they may furnish for elucidating the history of our planet. Every addition to these stores will serve to enlarge and consolidate the basis on which a true theory of the earth, if such can be found, must necessarily rest.

Although the British Isles have been very attentively surveyed, and every thing relating to the geological features of some districts examined and made known, yet there are considerable portions of the country, and these very interesting, to which the researches of the geologist have not yet extended. To fill up one of these blanks is the object of the volume before us: the district it embraces is extensive and important; and such has been the labour of the two gentlemen who have undertaken the task, that they have with unremitting ardour explored the whole line of the Yorkshire coast, from the Humber to the Tees, visiting every part of the interior likely to throw light on the objects of their research. Scarcely a hill or a valley, a cliff or a chasm, remains unexamined; scarcely an alum rock, a coal pit or a quarry, or any other remarkable opening in the strata, has been left unvisited, and the result of their labours is now laid before the public in a well-written memoir, illustrated by such engravings as fully explain the subjects referred to in the text.

The work is divided into three parts. The first consists of a minute description of the strata; the second part is devoted to an account of the organic remains discovered; and the third part consists of general observations, with such facts and inferences, hints and conjectures, as their labours have suggested.

The limits of a Magazine are much too narrow to do justice to a work of this nature, either in the way of analysis or extract: we shall therefore content ourselves with quoting from the facts and inferences some observations of the authors on the hypothesis of successive creations or formations of strata, contended for by some geologists, but to which they are opposed. They say,

"Of our fossil organized substances, some correspond with recent animals and vegetables, others have no recent analogues hitherto known; and these two classes are so intermixed, that we cannot regard the latter as more ancient than the former.— It is a fashionable opinion among geologists, that the animals and vegetables imbedded in rocks, are more or less ancient, and differ more or less from the present animals and vegetables, according as they are lower or higher in the series of strata. Such authors speak of different races being successively created and destroyed;

destroyed; each succeeding race approaching nearer to the present genera and species. According to them, there was first a world of zoophytes; and this being destroyed was followed by a world of cockles, or such like bivalves; which cockle world being also ruined, was succeeded by a world of crocodiles or huge lizards, destined to perish in their turn, to make way for other creations; a few stragglers from each lower world being allowed, however, to ascend and hold a station in the world next above: but all the inhabitants of these numerous worlds became extinct, before the creation of man, and his present fellow-tenants of the globe. Some go so far as to assert, that not one fossil species agrees exactly with any living species; except a few species found in the alluvium, which by peculiar favour have obtained a kind of *apotheosis*, having ascended from the world last destroyed, to figure in the present.—These notions, which seem to have gained currency chiefly through their novelty and their wildness, it is impossible to reconcile with facts. No such gradation exists; but we see in all the beds, whether high or low, organized substances that have recent analogues, and others that have not; and find as large a proportion of the latter in the oolite and the chalk, as in the aluminous strata. Zoophytes abound most in the chalk and the oolite, while in the lowest shale we see oysters and other shells, corresponding in every respect with living species. Indeed, there are some shells, particularly ostracites, ammonites, and belemnites, that exist in almost all the strata containing organic remains. They occur in the chalk and the oolite, and in the lowest shale: nay, they occur in much *lower*, or, to use the common phrase, *older* strata; for Dr. Macculloch discovered belemnites in Garvh island, in limestone alternating with gneiss and quartz rock*. The idea, that none of our fossil animals or vegetables can be assigned to any recent species, cannot be admitted, without shutting our eyes against the clearest evidence; and several genera and species now regarded as extinct, may yet be found recent. Many countries, rivers, and creeks, remain to be explored; and doubtless the ocean contains living treasures hitherto unseen. Brown in his Travels in Egypt, &c. (p. 70), observes, that, with the exception of some eels, none of the fishes which he found in the Nile correspond with the European fishes: and every scientific traveller discovers in distant parts of the world, new species, and even new genera, of animals

* Description of the Western Islands, ii. p. 512. It is worthy of notice, that Dr. M. observed in Rasay and Sky, a series of strata, reposing on gray wacké schist, conglomerate, and gneiss, bearing a strong analogy to part of our strata; consisting of white sandstone, dark blue shale with thin seams of coarse limestone, and below that, red sandstone. The shale contains ammonites, ostracites, gryphites, belemnites, &c. Ibid. i. p. 250, &c.

and

and plants. Within these few years, the *trigonia*, which was deemed an extinct genus, has been found recent; and the same remark is applicable to a few other genera. After the recent accessions which natural history has acquired, particularly the discovery of the ornithorhynchus and of the animal of Stronsay, we need not despair of seeing the lizard-fish in a living state.

"The authors of the hypothesis of successive creations, or *formations* as they are more frequently termed, have not told us what we are to make of the extensive strata containing no organic remains, or next to none, intervening between strata that abound with them. Was the creative power suspended or contracted for some ages? Did worlds of barren sand alternate with worlds replete with life?

"We have other objections to produce against this theory, but they will appear with more advantage under the next observation.

"We have reason to believe, from the facts before us, that no considerable interval occurred between the deposition of the several members of our strata; but that they were all deposited nearly about the same period.—The doctrine of successive formations is connected with the opinion, that ages intervened between one formation and another; and that the lowest strata are of very high antiquity, while the upper strata, such as the chalk beds, are comparatively quite modern. To the same system belong the notions, which we have already exploded, that the animals petrified in the several formations are peculiar to these formations, and that they have lived and died on the spots where we find them.

"As the formation system has many learned and zealous advocates, it is the more necessary to set forth the leading facts, from which we draw the conclusion, That the different members of our strata have been all deposited nearly about the same period.

"(1.) The breaks in the strata are not limited by the boundaries of any particular member of the series, but affect the whole mass of the strata at the places where they occur. Had the strata been deposited in successive formations, separated by ages, or long periods of time, we ought to find in the lower formations their own peculiar breaks and irregularities; and might expect to see, in numerous instances, breaks leaving off at the limits of the several formations; and to observe the materials of the higher formations descending into the fissures of the lower. Now, when we perceive, on the contrary, the same breaks passing directly through the aluminous beds, the coal measures, the oolite, and all the intermediate strata, without any regard to supposed formations, it is natural to conclude, that the division of the strata into such formations is the work of fancy. We do not, indeed, find any one break crossing the whole series; but we see a succession

cession of breaks connecting the different members, and showing the whole to be, not a series of formations, but one grand formation.—The effects of the denudations of the strata lead to the same conclusion; for the chalk, the upper shale and the oolite, have been swept away together between Speeton and Filey; and the aluminous beds and red sandstone have been involved in the same destruction towards the Tees.

"(2.) Most of the breaks or dislocations have taken place when the strata were but half consolidated; so hard as to break, yet so soft as also to bend. This fact deserves particular notice, as being, in our opinion, the most decisive evidence of the point in dispute; especially when viewed in connection with the fact last stated, and with the remarks made above (§ 12) on the induration of the strata. Had the strata been of different ages, we should have found at the breaks that pass through several members of the series, indications of the greater hardness of the lower beds, and softness of the upper, at the time when these breaks occurred. But, instead of this, we see in the bends, undulations, and contortions, accompanying the breaks, indubitable proofs, that the beds which are now the hardest were capable of being bent at the era of these dislocations, and the lowest as much as the highest. The undulations in the ironstone and hard sandstone on both sides of Scarborough; those in the sandstone at Haiburn wyke; those in the hard bands of the aluminous strata at Peak and Robin Hood's Bay; and those in the dogger near Saltwick, on the east side of Whitby harbour, and in the sandstone on the west side, may be quoted as examples. They show, that as the great breaks on the coast run through the entire mass of the strata, wherever they occur, so they must have taken place when every part of the mass was somewhat flexible. In some instances, indeed, the curvature of a bed is partly owing to small cracks or rents; but independent of such cracks, there is a real bending of the mass of the stratum.—Even the denudations present appearances, indicating that they must have occurred when the strata were but half consolidated; for it is difficult to explain, on any other principle, the extent to which the hard strata have thus been demolished.—These facts it is impossible to reconcile with the formation system.

"(3.) The conformity and close succession of the strata, viewed in connection with their contents, also furnish insurmountable difficulties in the way of the system.—The members of the strata succeed each other so closely, and with so little appearance of interruptions, or long intervals, between their deposition, that the abettors of this theory must find it difficult, if not impossible, to determine where one formation ends, and another begins. The members of the series often run into one another. The chalk

might be deemed one of the most distinct formations; and yet we have seen that at its junction with the upper shale, there is a gradual transition of the one into the other, the clay growing chalky, and the chalk clayey. Similar appearances occur at the junction of other members of the series; and even where there is a distinct line of separation, the evenness of that line is a proof, that the inferior member has not lain so long uncovered by its successor, as to allow the hand of time, or accidental causes, to produce inequalities in its surface.—Besides, the contents of the strata do not accord with the formation system. If each member of the series was formed so leisurely, and if its animals expired on the spots which they occupy, why are almost all the larger petrifactions, particularly the large marine animals, so mangled and broken; often parted into a thousand pieces, and their fragments scattered in all directions?—Again, if the strata were formed in the way supposed, why do we find in so many of them, both low and high, masses or fragments of petrified wood? Why is there wood in the alum shale, the ironstone, and the oolite, as well as in the coal and sandstone strata? Had each world its own trees, as well as its own animals? Where are the soils in which the successive races of vegetables grew? And why are the plants and the shells, the trees and the fishes, of these numerous creations, blended together?—On the whole, the formation system may please the imagination, and give scope to the fancy, but it will not stand the test of an appeal to facts.

"The basaltic dyke bears such strong marks of having been composed of fused matter, thrust upwards through a fissure in the strata, by volcanic agency, or something akin to it, that we may reasonably presume, that such agency may have been employed in raising the strata out of the ocean in which they were deposited.—Some may think, that we should have placed this observation among our *conjectures*, rather than among *facts* and *inferences*: but the appearances of igneous origin presented by our whinstone dyke, and other similar dykes, are so strong, as nearly to reduce the matter to absolute certainty*. Had the fissure occupied by the whin dyke been filled from above, as some suppose, whence were the materials derived? There are no strata above

* The Rev. A. Sedgwick, Woodwardian Professor, Trinity College, Cambridge, examined the rocks of this coast a few months ago, and having paid particular attention to our basaltic dyke, and to some trap dykes near Newcastle, and in High Teesdale, was fully convinced, that the evidence for their igneous origin appears quite complete. Near Caldron Snout, he found the limestone, where it comes in contact with the trap, converted into a granular mass, in which you lose all trace of organic remains; but gradually recovering its usual texture at the distance of a few feet. The coal shale, under the same circumstances, is so indurated as to resemble a piece of Lydian stone.

capable

capable of filling it; and if we could suppose that all the higher members of the series once extended over the space through which the dyke runs, which of these strata could supply the requisite materials? Why are the numerous cracks and fissures, in the oolite and other strata, not filled with the same substance? And, since so many of the upper beds consist of limestone, why does the dyke contain so small a portion of calcareous matter? If the fissure was filled from above, by secretions from beds that have been washed away, why does it not every where reach the surface? Or rather, as it is harder than the strata washed away from it, why does it not every where stand up above the surface like a wall, as it does at Langbargh and some other places? Besides, why are its contents disposed in large oblong blocks, lying across the fissure; and not rather arranged in a stratified form, or suspended in stalactitic masses? Above all, why is the dyke throughout its whole length composed of crystallized matter, and that matter not at all affected by the nature of the various strata through which it passes? In its progress from Maybecks to Cockfield, it crosses the blue limestone and the sandstone strata above it, the coal measures of our hills, the aluminous strata, the red sandstone of Cleveland, the magnesian limestone, and the Durham coal measures, arriving at, or approaching, the metalliferous limestone; yet the diversified nature of the beds through which it runs has no effect on it. Now, as the substance and structure of the dyke are nearly uniform, and have no connection with the nature or composition of the beds which it traverses, we are compelled to think, that it is all derived from one common source, and that source not above but below. And when we also see along its course, effects produced by it, exactly corresponding with the effects of ignited matter, what are we to believe, but that its substance has been forced upwards in a state of igneous fusion?—Hence, as we have seen that this dyke is connected with slips or breaks of the strata, it is natural to conclude, that the same kind of agency which forced up ignited matter into fissures of the strata, may have been employed in raising the strata themselves, out of the ocean in which they were formed."

This work is embellished with a geological map, a section of the strata, and seventeen lithographic plates, coloured. Those who are fond of a fine plate, would probably have preferred that they should have been engraved on copper; but these, which are executed by one of the artists, give an excellent idea of the various subjects they are meant to illustrate, and perhaps more natural than finer engravings.

An Illustration of the Genus Cinchona; comprising Descriptions of all the Officinal Peruvian Barks, including several New Species, &c. &c. By Aylmer Bourke Lambert, Esq. F.R.S. A.S. and G.S. Vice President of the Linnean Society, &c. &c. 4to. pp. 180. London, 1821.

If, as will scarcely be doubted, the Bark is one of the most important plants in the whole vegetable kingdom, then every information respecting the varieties of its species, the places where it is to be met with, and the medicinal qualities it possesses, must be considered as a valuable addition to the knowledge hitherto attained on the subject. Since the first discovery of this valuable plant to the present moment, no one has devoted so much attention to it as Mr. Lambert. In 1797 he published a Description of the Genus *Cinchona*; but since that time so many additions have been made by the illustrious travellers Humboldt and Bonpland, as well as by the authors of the *Flora Peruviana*, that he has been induced to give some additional illustrations on the subject.

Mr. Lambert entirely confines himself to the botanical definitions of the species, and gives more correct diagnoses of the species than has hitherto been done. To these he has added an account of the Cinchona Forests of South America, from the German of Humboldt; a Memoir on the different species of Quinquina, by Mr. Laubert, translated from the French; and four dissertations on various plants of South America. These documents afford every information relative to the history and various qualities of Barks, and contain much that is valuable and interesting, not only to medical men, but to the general reader.

It would be foreign to our purpose to quote Mr. Lambert's synopsis and description of the various species of Cinchona, which are given with that care, minuteness, and scientific detail, which a skilful botanist alone could appreciate.

Recent Publications.

Tracts on Vaults and Bridges; containing Observations on the various Forms of Vaults, on the taking down and rebuilding London Bridge, and on the Principles of Arches; illustrated by extensive Tables of Bridges. Also containing the Principles of Pendent Bridges, with reference to the Catenary applied to the Menai Bridge, and a theoretical Investigation of the Catenary. With 30 engravings. 8vo. 20*s*.

The Use of the Blow-Pipe in Chemical Analyses, and in the Examination of Minerals. By J. J. Berzelius, Member of the Academy of Stockholm, &c. &c. and translated from the French of M. Fresnel, by J. G. Children, F.R.S. London and Edinburgh,

burgh, F.L.S. &c. &c. With a Sketch of Berzelius's System of Mineralogy; a Synoptic Table of the principal Characters of the Pure Earths and Metallic Oxides, before the Blow-pipe; and numerous Notes and Additions by the Translator.

Observations on Leonardo da Vinci's celebrated Picture of the Last Supper. By J. W. De Goethe, Author of Werther, &c. With an Introduction and Notes. By G. H. Noehden, LL.D. 4to.

Journal of an Expedition 1,400 Miles up the Orinoco, and 300 up the Arauca; with an Account of the Country, the Manners of the People, Military Operations, &c. By J. H. Robinson, late Surgeon in the Patriotic Army. 8vo. 15*s.*

A Letter to Daniel K. Sandford, Esq. Professor of Greek in the University of Glasgow, in answer to the Strictures of the Edinburgh Review on the Open Colleges of Oxford.—By a Member of a Close College. 2*s.* 6*d.*

A Comparative Estimate of the Mineral and Mosaical Geologies. By Granville Penn, Esq.

An Inquiry into the Opinions, Ancient and Modern, concerning Life and Organization. By John Barclay, M.D. Lecturer of Anatomy and Surgery, Fellow of the Royal College of Physicians. 8vo. 14*s.*

The Inverted Scheme of Copernicus, with the pretended Experiments upon which his Followers have founded their Hypothesis of Matter and Motion, compared with Facts; the Doctrine of the Formation of Worlds out of Atoms by the power of Gravity and Attraction, exposed as foolish, and completely refuted as false; the Divine System of the Universe proved by Astronomical Tables to be true. To which is prefixed, a Letter to Sir Humphry Davy, Bart., President of the Royal Society. By B. Prescot, Esq. 8vo. 7*s.*

A Universal Technological Dictionary; or, Familiar Explanation of the Terms used in all Arts and Sciences; containing Definitions drawn from Original Writers. By George Crabb, A.M. Parts I. and II. 4to. 9*s.* each.

LXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

March 14 and 21.—On these evenings a paper on the Alloys of Steel, by J. Stodart, Esq. F.R.S. and Mr. Faraday, Chemical Assistant to the Royal Institution, was read.

Satisfactory experiments on these alloys having been previously made on a small scale in the laboratory of the Royal Institution, they were extended for the purpose of manufacture, and the

the products proved equal, if not superior, to the smaller products of the laboratory.

The most valuable alloys formed with steel, were those formed with silver, platinum, rhodium, iridium, osmium and palladium, in the proportion of one hundredth of these metals, except silver, with which steel will combine only one five hundredth part; when more is fused, the metals form only a mechanical mixture. The alloys are applicable to every purpose for which good steel is employed, but the cost must ever prevent their general application.

Platinum and rhodium combine with steel in every proportion, forming with some of the higher proportions beautiful compounds, the colour favourable for metallic mirrors, and not subject to tarnish in the air.

ASTRONOMICAL SOCIETY OF LONDON.

April 12.—Various papers were read at this meeting. The first was a communication from Mr. Lambert, giving an account of the result of his calculations relative to the longitude and latitude of the Capitol, in the City of Washington. The second was a list of observations of the planets, during the period of their respective oppositions, in the preceding year: with computations of their longitudes and latitudes, with a view to correct the errors of the tables, from Mr. Groombridge. The third consisted of a variety of communications from Major-General Sir Thomas Brisbane: some relating to the determination of the position of several places, others detailing some observations on the magnetic needle; and all of them of considerable interest: but the most remarkable (because the most singular) was his account of an occultation of the planet Mercury by the Moon, which was observed *at sea;* and he adds that, at the emersion, the planet appeared to have *retrograded* 2′ on the disc of the Moon. The fourth was a communication from M. Littrow respecting the practicability of making use of the pole star, at any time that it is visible, for the purpose of determining the latitude of the place of observation: with a collection of useful tables. The next meeting of the Society will be on Friday, May 10th.

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

Since the last Report the following papers have been read:

On the Mineral Productions and Geology of the Parish of St. Just. By Joseph Carne, Esq. F.R.S. M.R.I.A. Member of the Society.

On some Advantages which Cornwall possesses for the Study of

of Geology, and on the Use which may be made of them. By John Hawkins, Esq. F.R.S. Honorary Member of the Society.

On Stratification, and on the external Configuration of the Granite of Cornwall. By John Forbes, M.D. Secretary of the Society.

On the Gwithian Sands. By Henry Boase, Esq. Treasurer of the Society.

On the Slaty Rocks of Cornwall, more particularly on those usually denominated Killas. By Dr. Forbes.

Additional Observations on the Temperature of Mines. By R. W. Fox, Esq. Member of the Society.

Notice on the Geology of Nice. By G. C. Fox, Esq. Member of the Society.

Some Account of the South American Mines. By the Rev. John Trevenen.

Some Account of the Mines of Pasco, in South America. By Mr. Richard Hodge. Communicated, with additional Observations, by Sir Christopher Hawkins, Bart. M.P. F.R.S. Member of the Society.

Some Account of the external Features (natural and artificial) of a Country, from which its Geological Structure may be inferred. By Dr. Forbes.

Notice of the Quantity of Copper raised in Great Britain and Ireland in the Year ending June, 1821. By Mr. Alfred Jenkyn, Member of the Society.

Notice of the Quantity of Tin raised in Cornwall in the Year ending June, 1821. By Joseph Carne, Esq. F.R.S.

INSTITUTE OF FRANCE.

SYNOPSIS OF GEOGRAPHICAL RESERCHES RESPECTING THE INTERIOR OF NORTHERN AFRICA, BY M. WALCKENAER.

The task assigned to the author by the Academy was to examine an itinerary from Tripoli to Timbuctou, translated by a French Morocco consul from the Arabic of the Cheyk-Hagg-Cassem; this was an aged agent that served as a guide to the caravans in their journeys to Timbuctou.

M. Silvestre de Sacy being in possession of another itinerary from Tripoli to Timbuctou, written in the vulgar Arabic, translated it at my request. The annexed words by the author, terminate his itinerary: "Composed by me, Mohammed, the son of Ali, the son of Foul; my father was a free citizen, my mother a black slave; my country is Teraoubez and Tomboctou."

These two itineraries are of considerable importance for the geography of Africa, and I intend to publish them*, accompanied

* It has not yet appeared, but is announced as on the eve of publication.

with

with a map or chart; this last differs in many essential points from all that have hitherto appeared.

The regions in the interior of Africa, known by the name of Soudan, are rich and abundant in gold and ivory, and fertilized by large rivers and considerable lakes, interspersed with an immense population.

Mahometanism, which has overthrown and founded so many states, kingdoms, and empires, has effected important revolutions in the centre of Africa. The northern parts of the continent bordering on the Mediterranean were from very ancient times inhabited by civilized nations: and the Phœnicians, Carthaginians, Greeks, and Romans, flourished there in commerce and the arts, while the tribes of the interior, separated by vast barren spaces, remained barbarous.

Mahometanism, in subjecting all the north of Africa to a nation accustomed to traverse immense deserts, has proved a potent cause of civilization. The Arabs transported the camel with them into Africa, and the Moors that led a wandering life and had issued originally from Arabia, hailed their conquerors, whose language and customs were similar, as compatriots and not as usurpers. Till then, obstacles almost insurmountable were opposed to any civilized nation that would penetrate into the Soudan.

The Arabs without difficulty commenced a direct intercourse with the rich regions beyond the great Desert, and from which gold had long been deported. They sent regular caravans, which appear to have passed at first through the Fezzan and Agadez, as in that direction the Desert is intersected by a considerable number of oases, or fertile spots insulated in the midst of sands. But afterwards, when the empire of the Khaliphs had extended to the western extremities of Africa, and even into Spain, other caravans took a direction through the valleys of Sus, Darah, and Tafilet, which lie to the south of the kingdom of Morocco.

Colonies of Moors and Arabs were speedily established in various regions, and zealous missionaries penetrated into them. Human sacrifices were abolished, and the religion of Mahomet was a commencement of civilization among the Negroes. This horrid superstition, however, is still practised in countries more to the south, approximating to the Gold Coast, to Guinea, and to Congo.

The empire of the Khaliphs had its revolutions, and these, together with the wars between the Spanish Khaliphs and the African of the dynasty of Zeirites produced more frequent transmigrations to the countries beyond the great Desert.

LXIX. *Intelligence and Miscellaneous Articles.*

THE SOUTH-SEA ISLANDERS.

Captain Thomas Manby, who has been a voyage round the world, states, that he is enabled to prove that all the islands in the South Seas are peopled from the same stock; the language much resembles, and the same hieroglyphical characters are understood from one extreme of the Pacific Ocean to the other. As a proof of it, Capt. Manby submitted to be tatooed at Otahieta, and received from the king and queen the investiture of the highest honours they could bestow; which is, a circle or garter below the knee of the left leg, and a star nearly resembling a Maltese cross, beautifully executed on the skin, with other devices, which hieroglyphically related a curious adventure never to be effaced or forgotten. On leaving Otahieta, Captain Manby proceeded to Owhyhee, the largest of the Sandwich Islands, a distance of near three thousand miles, where every hieroglyphical character, tatooed on him, was deciphered most accurately by an old priest belonging to the Morai of King Tomahamaha, who related every circumstance with wonderful exactness, which greatly amused the king and all his queens, who made the Captain many valuable presents, and all showed him the most marked attention during the time he remained at the island.

At all the other islands, the same true and exact translation was always given, and created the greatest mirth wherever the characters were read; and such was the amusement it afforded, that the islanders would often watch for the Captain bathing to read an adventure which afforded many good-humoured jokes.

Captain Manby having obtained the interpretation of several hundred characters of an hieroglyphical nature, he intends speedily to publish them, which must prove of the utmost utility to future navigators, and throw a new light on the history of the innumerable islands that lie scattered over the immense surface of the great Pacific Ocean.

ON RESPIRATION.

On Tuesday the 16th instant, Dr. Roget gave his eighth lecture on Comparative Physiology, at the Royal Institution. In this lecture he took a comprehensive view of the subject of Respiration. The necessity of this function, he remarked, would scarcely have been anticipated, from our previous notions of the wants of an animal, founded on the known properties of organized matter; and yet observation shows, that the continuance of life is more immediately dependent on respiration, than even on the circulation itself. Insects, for example, that live without any

any vascular circulation of their juices, require the free introduction of air into every part of their bodies. The necessity for air appears, also, to be more urgent than for food; since animals may subsist a considerable time without nourishment, but all will speedily perish if deprived of air. The results of Spallanzani's numerous experiments were stated in illustration of this principle.

Aquatic animals being precluded from the benefit of the direct action of the air in its gaseous state, or as it exists in the atmosphere, receive its influence through the medium of the surrounding water, by which it is absorbed in large quantities, and applied to the organs of respiration. In the lower Zoophytes, this influence appears to be exerted by the intervention of the surface of the body: so that in the Polypus, for example, while the interior surface digests the food, and performs the office of a stomach, the external surface probably acts as an organ of respiration. Many of the Vermes appear, in like manner, to have an external respiration: this is the case with the leech and the earth-worm, in which a superficial net-work of vessels receives the influence of the surrounding fluid. In some genera of this class, it was stated, this structure is confined to particular parts of the surface; and in others, again, the respiratory organs shoot out from the body in the form of bushy fibrils. The different situations of these arborescent gills, which are frequently kept in incessant motion, were pointed out in several orders of molluscous and crustaceous animals.

Dr. Roget then proceeded to examine the extensive series of animals in whom respiration takes place in the interior of the body: beginning with the *holothuria*; the ramified tubes of which exhibit the first trace of a structure adapted to this object; the *asteria*, and the *echinus*, in which the arrangement is somewhat more complicated; and the larger *Crustacea*, as the lobster and crab, in which the filaments are collected into a number of pyramidal organs on each side of the body, protected by the shell, and terminating with the more regular structure of gills proper to the ordinary Mollusca, and Fishes. The disposition of these organs, with reference to the shell, and to the apertures in the mantle, by which the water is admitted to them; and the provision of tubes, capable of being extended and retracted, in those shell-fish that burrow in the sand; were severally pointed out and described. The two auxiliary hearts of the cuttle-fish, at the origin of the branchial arteries, by which the blood of that animal is propelled with force to the respiratory organs, while the principal heart carries on the aortic or greater circulation, were particularly noticed.

The importance of the respiratory functions increases as we

rise

rise in the scale of animals. In Fishes, the gills form a considerable portion of the system, and their office appears to be more essential to life than in the *Mollusca.* The situation and structure of these organs were minutely described, together with the mechanism by which their action is maintained. The air contained in the water is equally vitiated by the respiration of fishes, and requires an equally constant renewal as in terrestrial animals. Fishes are, therefore, killed in a short time, if confined in a limited portion of water which has no access to fresh air. When many fish are inclosed in a narrow vessel, they all struggle for the uppermost place, where the atmospheric air is first absorbed, like the unfortunate men imprisoned in the black hole at Calcutta. In Humboldt and Provençal's experiments, a tench was found to be able to breathe when the quantity of oxygen in the water was reduced to the five-thousandth part of its bulk, though it is in this way brought into a state of extreme debility: but the fact itself shows the great perfection of the organs in this fish, that can extract so minute a quantity of air from water, to which the last portions always adhere with great tenacity.

The respiration of air in its gaseous state is performed by breathing terrestrial animals in two ways: first, by means of tracheæ, a mode peculiar to insects; and secondly, by pulmonary cavities, which constitute the essential structure of lungs. The tracheæ of insects are tubes which take their rise by open orifices, called spiracles or stigmata, from the surface of the body, and are distributed by extensive ramifications to every part. They extend even to the wings, to the sudden expansion of which they appear to contribute. In the higher classes of articulated animals, as soon as blood-vessels are met with, the whole apparatus of tracheæ is found to disappear; their necessity being superseded by the power, derived from the possession of circulating vessels, of transmitting the juices to particular organs, where their exposure to the influence of the air may be conveniently effected. The pulmonary cavities of spiders, and of some gasteropodous Mollusca, such as the snail and slug, which breathe atmospheric air, are of this description.

The structure of the pulmonary organs becomes more refined and complex as we proceed to the higher classes of animals. Dr. Roget entered into a description of these various structures, and of the diversified modes in which the air was received, and made to act upon them, and afterwards expelled, in the different orders of reptiles, of mammalia, and of birds. The singular mode in which the frog swallows its air, and inflates its lungs at pleasure, was pointed out. The dilatation of the chest in man, and the other mammalia, by the muscular action of the diaphragm, and

by the movements of the ribs, during inspiration, and its contraction during exspiration, were fully explained, and partly illustrated by a machine, which exemplified the effects of the motion of the diaphragm. This part of the subject was concluded by an account of the peculiar mechanism of respiration in birds, by which the same air is made to pass twice through the lungs, before it is finally ejected from the system; being received into large cells, which inclose all the principal organs, and even pervade the muscles, and subcutaneous membrane.

Dr. Roget next gave a brief account of the chemical changes effected in the blood, which is exposed to the action of the air during respiration. Our knowledge of these changes, he remarked, was not so much derived from the direct analysis of that fluid in its different states of venous and arterial, as from the inferences necessarily to be drawn from the changes found to have occurred in the air by its passage through the lungs. These changes consist in the disappearance of a quantity of oxygen, and the addition of a corresponding quantity of carbonic acid, and of watery vapour. The redundant carbonaceous principle which accumulates in venous blood in the course of the circulation, is thus discharged in the lungs by its combination with oxygen, and the blood is restored to the vivifying arterial qualities. The analogies between this process, and that of slow combustion, were pointed out, and extended to the phænomena of the high temperature which so many animals maintain above the surrounding media, and which establishes so striking a distinction between warm- and cold-blooded animals, more especially remarkable among the larger inhabitants of the ocean.

ANTIDOTE FOR VEGETABLE POISONS.

M. Drapiez has ascertained by numerous experiments, that the fruit of *Feuillea cordifolia* is a powerful antidote against vegetable poisons. He poisoned dogs by the *Rhus Toxicodendron* (Swamp Sumac), Hemlock, and Nux Vomica. All those that were left to the poison died; but those to which the *Feuillea* was administered recovered completely, after a short illness. —*American Paper*.

IMPROVED PREPARATION OF COAL FOR FUEL.

Mr. Peter Davey, of Old Swan Wharf, Chelsea, has obtained a patent for an improved preparation of coal for fuel. It is called "gaseous coke," and consists of "very small coal mixed with coal tar, either in a pure state, which is the best, or combined with naphtha, and those other ingredients with which it is generally

rally found impregnated." These materials are made to coagulate and cement together by the application of heat, so as to form large cakes, capable of being broken into lumps of such sizes as may be found convenient for the purpose of fuel.

WORM-PROOF TIMBER.

What has been so long and so ardently sought for by shipbuilders, we believe to be now nearly if not wholly attained. We allude to the discovery of timber which will secure a ship's bottom against the terrible invasion of the *worm*, so universally destructive.

This discovery was accidentally made by Captain Thomas Shields, during his residence at the bay of St. Louis. He found that a particular stake, used for fastening a boat, had remained perfectly good and staunch for a year; whereas others had to be replaced every two or three months, being destroyed by the worm. On examination, this stake proved to be of Sweet Gum, a timber usually considered of no value. Captain S. deciding to make a full and fair experiment, procured a small tree of the sweet gum, hewed it down until it squared nine inches, and then had it staked in three feet water, affording every opportunity to the *worm*. This sweet gum stick remained thus exposed for four years; when on examination it was found perfectly free from moss, barnacle, and all other excrescence; and on hewing it down again an inch or more, no traces of the worm were to be seen, except three or four very small punctures of inconsiderable depth. Captain Shields communicated these facts to Commodore Patterson some years ago. The Commodore declared his intention of making a further experiment in the Lake Barataria. Whether this was done, or what was the result, we know not; but we hope the experiment, if made, was as satisfactory as that at Bay St. Louis.

The Sweet Gum [*Liquidambar styraciflua* Linn.] is in great abundance on the Alabama, and the lakes and bays between Pensacola and New-Orleans—it is of prodigious girth and towering tallness—frequently exhibiting a smooth stem of 50 or 60 feet, and remarkably straight. It can be sawed into planks of almost any size, but it will not split—on which account it is universally rejected as useless.

Is it not worth the experiment? Cut this timber into sheathing plank, of half inch or less, and try it on some of our lake craft. Its flexibility is such, that a thin plank may be bent and shaped almost as one pleases.—*The Floridian, March* 10.

TO RESTORE OLD APPLE TREES.

A gentleman at Littlebury in Essex, having in his orchard many old supposed worn-out apple trees, which produced fruit

scarcely

scarcely larger than a walnut, last winter took fresh made lime from the kiln, slacked it with water, and (without allowing time for its caustic quality being injured by imbibing fixed air) well dressed the trees, applying the lime with a brush. The result was, that the insects and moss were completely destroyed, the outer rind fell off, and a new, smooth, clear, healthy one formed; and the trees, although some twenty years old, have now a most healthy appearance.

It will readily occur to the reader, that the same treatment may be extended to other fruit-bearing trees, and probably with a similar beneficial result.

PRINTING PRESS FOR THE BLIND.

A Journal printed at Geneva thus announces a very interesting invention:—A Press for the Blind.—A lady deprived of sight from her birth, but distinguished for her wit, her talents, and good temper, conceived that it might be possible to communicate her thoughts to her family and friends by means of printing, if some skilful mechanic would invent for her a press, and give her the necessary instruction to make use of it—the application and patience for its accomplishment becoming afterwards entirely her own. She addressed herself to our countryman Mons. François Huber, the celebrated historian of Bees, to whom she had the advantage of being related; in addition to which, a community of misfortune (for he also is blind) increased the interest he had in gratifying her request. Thereupon his own genius, and that of his servant Claude Lechet, a man endowed with the highest degree of natural talent for mechanics, were strongly excited. They went to work, and the press was invented; and being finished by Claude, who sent with it a collection of types to the amiable suggester of the plan, she soon made herself mistress most completely of this invaluable means of communicating her ideas. We have seen a letter of 33 lines addressed to her happy benefactor, composed, and printed by herself with common ink, without a literal error, or a single typographical irregularity."—*Courier de Londres, April* 5, 1822.

ERUPTION OF MOUNT VESUVIUS.

Naples, Feb. 25.—On the 13th of this month two loud subterraneous detonations were heard in the neighbouring communes of Vesuvius; these phænomena usually precede each eruption. From the night of the 16th to the 17th the detonations were renewed with violence, and were heard from hence. On the following day it emitted a thick smoke; on the 19th it began to throw up a shower of cinders and stones, and soon after fragments of inflamed lava. This eruption again covered the whole extent of the *crater*, a width

width of about twenty toises, forming a crown of fire. For the two following days the eruption became more violent, and the boiling lava, which was filling the *crater* and threatening at every moment to break over its sides, was seen distinctly during the night. At length, on the 21st, the lava forced its way into the southern part of the mountain by a new opening, from which it flowed in great abundance. The flowing took its direction slowly (it ran a toise a minute) towards the hermitage of Saint Salvator. During the two following days the same phænomena succeeded, without interruption, but without any increase of force. Yesterday, towards ten o'clock in the morning, the violence of the eruption was suddenly redoubled. The lava, which continued flowing in the same direction, when it reached the territory of the Cantroni, turned its course towards the west, and precipitated itself into a valley. In the evening Vesuvius presented to the inhabitants of Naples the superb spectacle of a river of fire, rolling down the skirts of the mountain, through clouds of smoke. A brilliant flame arose from the crater, and nothing troubled this splendid evening, not even the fears and disasters which too often accompany this terrible phænomenon. This time the lava took its direction through lands already burned and entirely desert, and no property appeared threatened with desolation. Vesuvius seems calm to-day, but a brilliant sun prevents us from discovering what is passing on the mountain.—*Paris Paper.*

ENCROACHMENT OF THE SEA.

On the east coast of America the sea appears to encroach upon the land more and more from north to south. At Cape May, where the Delaware falls into the Atlantic Ocean, a house is built on the wall of which are inscribed the following important observations:—

Distance of the sea from the house.

	Feet.		Feet.
1804	334	1812	254
1806	324	1816	225
1807	294	1817	214
1808	273	1818	204
1809	267	1819	188
1811	259	1820	180

The inhabitants of the coast of Brazil say that they have made similar observations, but we have no particulars of them. There is a building at Ilheos, which was formerly at a good distance from the sea shore, but is now scarcely a hundred steps from the breakers.

AFRICA.

AFRICA.

Accounts from Sierra Leone to the middle of January state, that a deputation had arrived there from Almamy Abdal Kader, king of the Toulahas, at the head of which was a prince, and a Mahomedan priest accompanied by his wife. The priest came all the way from Egypt to the Mandingo nation, and had procured important information of the geography of Oriental Africa; he had passed through Timbuctoo, and is of opinion that the Niger and the Nile are the same river. The kingdom of Toulaha, with which an intercourse has thus been opened, is only a few days journey from the Niger.

WINTER IN THE NORTH.

Christiana, Feb. 20.

We have had a most extraordinary winter; no snow, seldom frost at night, and generally several degrees of heat. In our country, where so much depends on winter, this may be considered as a national calamity; in fact, we hear complaints on all sides. The inhabitants of the town suffer greatly, because they can receive no provisions, the prices of which daily rise.

St. Petersburg, Feb. 20.

We have the mildest spring weather. The ice mountains, the favourite amusement of the Russians in Lent, could not be erected as usual on the Neva, because the ice was not strong enough to bear the weight.

A heavy rain fell at Beseroso on the 16th of December, a circumstance unparalleled at that time of the year in so high a latitude.

St. Petersburg, March 20.

Winter, which has this year formed one of the most extraordinary phenomena in the northern countries known in the physical world, and of which modern history does not afford a parallel instance, ought in consequence to be noticed in its annals. Our winters are generally very severe during four successive months, and they are, though more moderate, yet still severe two other months. The total duration of our winter is about six months more or less; but that of the present year has been but one month and a few days. The first snow fell on Christmas day, and it had generally disappeared in the beginning of February. Since then we have had a mild temperature, with some days rain, and on others snow. The general serenity of the atmosphere was however disturbed by violent tempests, and a wind from the south-west, which swelled the canals, and by the inundations threatened the lower part of the city with great danger.

Winter corn has been much injured on the coasts of the Baltic and in White Russia, on account of the humidity of the soil, and the

the cultivator has no hopes of a good crop. The news from the interior of the empire of the effects of the winter, are equally unfavourable. In the southern provinces there, it had been colder than here, but it was unaccompanied with snow; and the thaw commenced in the middle of January. The Duna was clear of ice on the 2nd of March, and, what is unusual, the breaking up of the frost did no damage. The navigation is open at Riga, and an English vessel has already arrived in that port from Hull.

In Siberia, where winter is constantly severe, the weather has been comparatively temperate; warm winds have been prevalent at Tobolsk, and to the north-east. Above all, the snow is already gone. At Bereson, one of the most northern cities in this country, it rained heavily on the 28th of December, a circumstance never before known by the oldest inhabitant.

WINTER IN SOUTH AMERICA.

Letters from Buenos Ayres, dated the 20th February, state that in the month of December last there fell such a quantity of snow, that the communication between that city and Lima was entirely interrupted. The cold that had been felt in the several countries of Southern America, is a most extraordinary phenomenon, and the inhabitants of Peru and Chili consider it as an awful calamity.

VOLCANO IN ICELAND.

While the winter in the east of Europe has been remarkably mild, it set in early in Iceland with great rigour. Vast quantities of snow fell, and the northern and eastern coasts were wholly blocked up with floating ice. In the night of the 20th December, the mountain Oefelds Jokel, to the south-east of Hecla, which has been at rest ever since 1612, began to emit fire, so that the ice with which it was covered suddenly burst with a dreadful crash, the earth trembled, and immense masses of snow rolled from the summit of the mountain, a height of 5500 feet. Ever since, a large column of fire has been rising from the mountain, which threw out vast quantities of ashes and stones, some of the latter weighing from 50 to 80 pounds, being cast to the distance of a German mile (five English miles). The mountain continued to burn till the 1st of February, and smoked till the 23d, but at that time the ice had again collected round the crater. The weather was very stormy during the eruptions.

CURRENTS IN THE OCEAN.

On the 6th of April, Mr. Hall, who occupies a farm situated on the south side of Milford Haven, picked up a bottle inclosing a paper, of which the following is a copy:

"No. 310.—The bottle which contains this card was thrown into the sea in lat. 49. 54. north; long. 12. 20. west, at noon,

on the 1st of March 1822, from the ship Ospray, of Glasgow, which sailed from Greenock on the 20th day of February 1820, on a trading voyage round the world. Whoever finds this is requested to insert a notice of the time and place in some literary or political publication, with the view of establishing facts relative to the currents of the ocean: 130 days from Calcutta, returning towards Greenock—'All well.'

"ALEXANDER M'GILL, Master."

LIZARDS OF CASHAU.

"I observed numbers of lizards and tortoises crawling along the sides of the road. Some of the former were of strange shapes, and others of unusual length; one that we found dead, was above two feet from the nose to the tip of the tail. I remarked that these animals invariably took the colour of the ground on which each particular kind existed. If verdant, the lizard was green; if sandy, she was yellowish white; if red earth, or reddish mouldering stones, she was pink; and if found among fragments of rock, and other dusky hued relics, she would appear of a varied brown. I leave this fact to naturalists to explain, confessing myself totally ignorant of its secret."—*Sir Robert Ker Porter.* Travels, vol. i. p. 390.

ATMOSPHERIC PHENOMENA.

To the Editor.

SIR,—The large halo so frequently seen in particular states of the atmosphere, at the distance of about 22 degrees from the apparent disk of the sun or moon, has been very properly called the *opprobrium philosophorum*. Newton himself, who so satisfactorily solved the phenomena of the rainbow by the principle of ordinary aqueous refraction, and those of the smaller haloes or coronæ by that of refraction through thin strata, acknowledges that the great halo is of a distinct kind. He could only explain it by falling in with the idea then prevalent, that it was produced by refraction through a horizontal stratum of hail or snow; a notion which has been regarded as very doubtful, from considering both the great variety of atmospheric temperature at which this appearance occurs, and the very peculiar structure which, on calculation, it has been found necessary to attribute to the floating icy particles.

A late writer has advanced as an hypothesis, that the refractive power of particles of vapour sufficiently small for permanent floatage in the atmosphere is diminished by the attraction of the air; an hypothesis by no means improbable, since the efficacy of the attraction in question follows the inverse ratio of the diameter of the particle.

Possibly

Possibly some light may be thrown upon the subject by the solutions, which may this year be expected, of a problem regarding coronæ, which was proposed two years ago by a distinguished institution on the continent; supported as the arguments probably will be, by the recent improvements in the theory of light.

The inquiry is doubtless rendered still more perplexing on account of the great halo being accompanied, much more frequently (according to the observations of Dr. Burney) than is generally supposed, by the appearance of *parhelia* or mock-suns. These are most usually situated in the circumference of the great halo, at the same height above the horizon as the real sun, and are connected with a horizontal beam of white light extending to a greater or smaller distance in a direction opposite to the sun. In fact, two classes of coincident phenomena are to be accounted for; viz. coronæ having the sun, or moon, for their common centre, and luminous circles or arches parallel to the horizon. The former, when observed by reflection from still water, appear sometimes very numerous. The latter are of rare occurrence, if we may judge from the small number of recorded observations.

These reflections were elicited by the appearance of a very interesting combination of such phenomena, which I had the pleasure of witnessing yesterday in the vicinity of this city. The halo was very perfect, increasing in brightness towards the summit. The sun shone with dazzling brightness through a light stratum of vapours. Both parhelia were very bright, coloured on the side next the sun, and attended by horizontal trains of ten degrees or more in length. But the most beautiful object was a *brilliant reverted rainbow*, about a quarter of a circle in extent, whose centre seemed to coincide with the zenith, and its vertex was on the sun's azimuth. The time was about 4 *p. m.* the barometer standing at $30\frac{1}{2}$, and the thermometer in a north room at 57°.

A similar rainbow, in the same position with regard to the zenith and the sun, I happened to see at sun-set about eighteen months ago. I am Sir, &c.

Grosvenor-Place, Bath; March 30. W. G. H.

April 1.—Last evening, a little before nine o'clock, I had an opportunity of observing a similar combination of circles about the moon, viz. the great halo and a complete horizontal circle of white light passing through the apparent place of the moon. Both this and the former phenomenon occurred in strata of vapour rapidly passing into the cirrostratus form; and taken in connection with the change of weather, from S.W. winds and mildness, to N.E. stormy and frost, certainly seem to favour the notion alluded to above, which ascribes these appearances to the reflecting and refracting powers of frozen vapour.

BIOGRAPHY.—EDWARD DANIEL CLARKE, LL.D.

There are few names better or more extensively known than Edward Daniel Clarke, who by his travels has rendered himself celebrated not only in Europe but in every quarter of the civilized world. Dr. Clarke was of a literary family; his maternal grandfather was the very eminent Dr. William Wotton, well known in the literary world by his proficiency when an infant in a great variety of languages; and his grandfather and grandmother were happily designated by the poet Hayley, in an affectionate epitaph, as

"Auld William Clarke, and Anne his wife."

Mr. Clarke, among other works, published "The Connexion of the Roman, Saxon, and English Coins." His son, the father of the subject of the present memoir, the Rev. Edward Clarke, was like his father a man of genius and a scholar.

Edward Daniel Clarke was born in the year 1769, and was educated at Jesus College, Cambridge; in 1790 he took the degree of B.A.; in 1794 M.A., and became senior fellow of that College. Soon after taking his degree, Dr. Clarke accompanied the present Lord Berwick abroad, and remained for some time in Italy. The classic scenes he there met with, and his own inquisitive genius, stimulated him to enter into a wider field of research; and shortly after his return to England, he embarked on those travels which have rendered his name so distinguished. To enter into any description of them is needless—they are before the public. They have been, and will continue to be, the delight and solace of those who have been unable to visit other countries; and they have excited the dormant spirit of curiosity in many a resident of the University, who has followed eagerly the steps of Dr. Clarke, and has invariably borne testimony to the accuracy and fidelity of his narrative. Dr. Clarke has somewhere mentioned all the excellencies which must unite to form a perfect traveller—he must have the pencil of Norden, the pen of Volney, the learning of Pococke, the perseverance of Bruce, the enthusiasm of Savary. Of all these Dr. Clarke united in his own person by far the greater share. No difficulties in his progress were ever allowed to be insuperable; and upon all occasions he imparted to others a portion of his own enthusiasm. It was upon the return from this extensive tour, during which he had visited nearly the whole of Europe, and parts of Asia and Africa, that Dr. Clarke presented to the University those memorials of his travels, which now decorate the vestibule of the library; and as some return for the splendour which his name had reflected upon the University, he was complimented in full Senate with the degree of LL.D. Among the contributions to the University,

versity, the most distinguished are the celebrated MS. of the works of Plato, with nearly 100 other volumes of manuscripts, and the colossal statue of the Eleusinian, respecting which Dr. Clarke published a very learned treatise, upon its being placed in the vestibule of the University library; but that which added most to his literary reputation, was a "Dissertation on the famous Sarcophagus in the British Museum," which Dr. Clarke caused to be surrendered to the British army in Egypt, and which he has proved from accumulated evidence to have been the tomb of Alexander.

During his travels Dr. Clarke made a very large and valuable collection of minerals, which it is thought will be purchased by the University. A rare and valuable assortment of plants, likewise, several of which were procured from the celebrated Professor Pallas in the Crimea, distinguished the industry and taste of this gentleman. Greek medals also engaged his attention when he was abroad; and many which adorned his cabinet are of singular rarity. Lord Berwick has in his possession a curious model of Mount Vesuvius, formed on the spot by Dr. Clarke, with the assistance of an Italian artist, of the very materials of the mountain.

In 1806 Dr. Clarke commenced lectures on mineralogy in the University of Cambridge; and when in 1808 a professorship was founded for the encouragement of that science, he was appointed to the chair. These lectures have, if possible, made his name more known and honoured, both in this and in foreign countries, than even his long and interesting travels. Natural history was his earliest and most favourite study; and that peculiar branch of it, which refers to the mineral kingdom, soon engrossed the whole of his attention. In the delivery of his celebrated lectures, Clarke was without a rival—his eloquence was inferior to none; (in native eloquence, perhaps, few have ever equalled him in this country;) his knowledge of his subject was extensive; his elucidation clear and simple; and in the illustrations which were practically afforded by the various and beautiful specimens of his minerals, he was peculiarly happy. Most of those specimens he had himself collected, and they seldom failed to give rise to the most pleasing associations by their individual locality. We may justly apply to him in the delivery of his lectures, what is engraven on the monument of Goldsmith, "*Nihil, quod tetigit, non ornavit.*"—Of the higher qualities of his mind, of his force and energy as a Christian preacher, of the sublimity and excellence of his discourses, we might tell in any other place than Cambridge; but there all mention of them is unnecessary, his crowded congregations were testimony sufficient. For the estimation in which Dr. Clarke was held by foreigners,

reigners, we may in the same manner refer our readers to the various honorary societies in which his name stands enrolled; and we may safely say, that to no one person has the University of Cambridge been more indebted for celebrity abroad during the last twenty years, than to her late librarian, Dr. Clarke. He has fallen a victim indeed to his generous ardour in the pursuit of science—he looked only to the fame of the University; and in his honest endeavours to exalt her reputation, he unhappily neglected his own invaluable health.

Perhaps no person ever possessed in a more eminent degree than Dr. Clarke, the delightful faculty of winning the hearts and riveting the affections of those into whose society he entered. From the first moment his conversation excited an interest that never abated. Those who knew him once, felt that they must love him always. The kindness of his manner, the anxiety he expressed for the welfare of others, his eagerness to make them feel happy and pleased with themselves, when united to the charms of his language, were irresistible. Such was Dr. Clarke in his private life—within the circle of his more immediate friends. In the midst of his family, there he might be seen as the indulgent parent, the affectionate husband, the warm, zealous, and sincere friend. Of his public life the present limits will only admit of an outline.

This much respected individual, whose health had long been in a declining state, died at the house of his father-in-law, Sir William Beaumaris Rush, Bart. in Pall Mall, on the 9th of March. In addition to his University offices, he was rector of Harlton in Cambridgeshire, and of Great Yeldham in Essex. The remains of Dr. Clarke were interred in Jesus College, Cambridge, on the 18th of March, preceded by the Master (the Vice Chancellor) and the Dean, and followed by his private friends, the fellows of the College, and many members of the Senate.

The works of Dr. Clarke were,

1. Testimony of different Authors respecting the Colossal Statue of Ceres, placed in the Vestibule of the Public Library at Cambridge, with an account of its removal from Eleusis. 1803.
2. The Tomb of Alexander, a dissertation on the Sarcophagus brought from Alexandria, and now in the British Museum. 1805.
3. A Methodical Distribution of the Mineral Kingdom. 1807.
4. A Letter to the Gentlemen of the British Museum. 1807.
5. Description of the Greek Marbles brought from the Shores of the Euxine, Archipelago and Mediterranean, and deposited in the Vestibule of the University Library, Cambridge. 1809.
6. Travels in Europe, Asia and Africa. 1810—1814.
7. A Letter to Herbert Marsh, D.D. in reply to Observations in his pamphlet on the British and Foreign Bible Society. 1811.

The

The last part of his Travels is now in the press, and the Doctor had nearly concluded it when his *valuable life closed. The* little that was left unfinished, it is said, can be *completed* from the ample memoranda left by the indefatigable and enterprising traveller.

LIST OF PATENTS FOR NEW INVENTIONS.

To George Stephenson, of Long Benton, Northumberland, engineer, for certain improvements in steam-engines.—Dated 21st March 1822.—2 months allowed to inroll specification.

To Richard Summers Harford, of Ebbw Vale Iron Works, in the parish of Aberystwith, Monmouth, iron-master (being one of the people called Quakers), for improvements in the heating processes in the manufacture of bar, rod, sheet, and other descriptions of malleable iron, whether the same may have been previously prepared by the pudling or other modes of refining.—21st March.—4 months.

To William Church, of Nelson-square, Surry, gentleman, for an improved apparatus for printing.—21st March.—6 months.

To Alexander Clark, of Dron, in the parish of Louchars, Fifeshire, esq. for an improvement in the boilers and condensers of steam-engines.—21st March.—6 months.

To William Pride, of Uley, Gloucestershire, engineer, for his self-regulating apparatus for spooling and warping woollen or other warps or chains, which invention he believes will be of much benefit and utility.—16th April.—2 months.

To William Daniel, of Abercarne, Monmouthshire, manufacturer of iron, for certain improvements in the rolling of iron into bars used for making or manufacturing tin plates.—16th April.—2 months.

To Benjamin Cook, of Birmingham, patent tube manufacturer, for a certain mixture or preparation, which may be used with advantage in preventing the danger of accidents from fire.—16th April.—6 months.

To John Grimshaw, of Bishopwearmouth, Durham, rope-maker (being one of the people called Quakers), for his method of stitching, lacing, or manufacturing of flat ropes, by means of certain rotative machinery connected with or worked by a steam-engine, or other rotative power, whereby the said stitching, lacing, or manufacturing of flat ropes is better executed than the same can be done or performed by any other method now in use, and which invention he apprehends will be of general benefit and of great public utility.—16th April.—2 months.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand.

Days of Month. 1822.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Weather.
Mar. 27	47	57	50	30·20	Fair
28	52	66	50	29·99	Fair
29	47	58	50	30·44	Fair
30	50	53	42	29·50	Showery
31	39	43	39	30·42	Sleet
April 1	40	46	42	·38	Fair
2	40	48	40	·35	Fair
3	41	52	46	·37	Cloudy
4	46	50	46	·23	Cloudy
5	46	52	46	·05	Cloudy
6	46	53	42	29·90	Fair
7	39	49	40	30·02	Hail-storm
8	40	48	36	·06	Ditto
9	35	48	36	·08	Ditto
0	33	44	38	·06	Ditto
11	40	46	41	29·99	Fair
12	42	50	46	·76	Rain
13	50	57	48	·94	Fair
14	48	58	49	30·04	Cloudy
15	50	61	47	·04	Cloudy
16	47	55	47	·04	Rain
17	48	54	46	29·92	Showery
18	46	52	44	·67	Cloudy
19	45	53	47	·74	Showery
20	45	54	48	·74	Fair
21	48	57	54	·50	Rain
22	50	58	46	·43	Showery
23	45	56	47	·34	Showery
24	50	57	48	·67	Fair
25	49	58	46	·58	Fair
26	47	57	50	30·02	Fair

N B. The Barometer's height is taken at one o'clock.

LXX. *An alphabetical Arrangement of the* Places *from whence* Fossil Shells *have been obtained by Mr.* James Sowerby, *and drawn and described in Volume* III. *of his "Mineral Conchology," with the geographical and stratigraphical Situations of those Places, and the Species of Fossil Shells, &c.* *By Mr.* John Farey, *Mineral Surveyor.*

To Dr. Tilloch.

Sir, — In your 46th volume, p. 211, and in your 52d volume, p. 53 are inserted, alphabetical Lists of the *Places*, mentioned as localities of the Fossil Shells figured and described by Mr. Sowerby, in the two first volumes of his "Mineral Conchology;" and his third volume having now been completed, I beg the favour of you to insert, a similar List of the Localities in this third volume: in which volume, 45 Genera, embracing 195 species or varieties of Shells, are described belonging, *each to some one particular Stratum;* and besides these, 19 other species or varieties* belonging to *some other Stratum*, and which Species are on that account distinguished, by adding the Greek Letters β, γ or δ, for the purposes of stratigraphical discrimination.

I feel a satisfaction in observing, that almost every succeeding Number of Mr. Sowerby's Work (which now appear *Monthly*), contains increasing evidence, in favour of those Principles regarding *Fossil Shells and their connexion with the Strata*, which I am somewhat proud of having developed and given to the world, in your 53d volume, p. 115, and in others of your pages.

My stratigraphical Index to Mr. Sowerby's 3d volume, has been sent to him for publication, corresponding to the List now sent to you: I beg to request of your Geological Readers to examine these, and to send corrections of the same to you, to Mr. Sowerby or myself, wherever they may be able to detect errors therein, truth alone being my aim.

In the List now sent, several errors recently discovered in the former Lists inserted in your Work, will be found corrected; but some others yet remain, in the List in vol. 52, which it may be proper to correct, as follows, viz.: p. 353, l. 9, *for* Blue Lias *read* Portland Rock: p. 355, l. 10 and 11, *for* Portland Rock *read* Coral Rag, and l. 22, *for* W *read* E: p. 356, l. 2,

* Three of these are already included in the List in vol. 53, p. 120, and the other 16 are as follows, viz. Ammonites annulatus 2 Species, A. Bechei 2, A. Koenigi 2, Avicula echinata 2, A. inequivalvis 3, Cardita margaritacea 2, Conularia quadrisulcata 2, Hippopodium ponderosum 2, Inoceramus sulcatus 2, Lutraria ovalis 2, Mya? literata 2, M. V-scripta 3, Terebellum fusiforme 2, and Terebratula inconstans 2 Species: at a future time I intend to particularize these, in the manner of the List in vol. 53.

for under Oolite *read* Lias? ; p. 358, l. 27, *for* Portland Rock *read* Coral Rag ; p. 359, *dele* l. 11, 12 and 13 ; *dele* l. 13 and 14 ; p. 362, l. 18, *for* under Oolite *read* Lias?, and l. 19, *for* α, *read* γ ; and p. 363, l. 4, before t *insert* *a*.

I am, sir, your obedient servant,

37, Howland-street, Fitzroy-square, JOHN FAREY.
15 April, 1821.

An alphabetical List of the Places from whence FOSSIL SHELLS *have been obtained by Mr.* JAMES SOWERBY, *and described and figured in Vol.* III. *of* MIN. CONCH. : *each referred to its proper* STRATUM *in Mr.* SMITH'S *Series, and in his Maps, Sections, and Geological Tables.*

Alderton, 6½ m SE of Woodbridge, Suff., in Crag Marl.
Cardium angustatum, t 283, f 2.

Alldown, see *Haldon*.

Alum Bay, at W extremity of Isle of Wight, in London Clay.
Cardita margaritacea β, t 297, f 2.

Ancliff (Avoncliff?) of Bath, Somerset., in Fullers' Earth Strata.
Modiola gibbosa t 211, f 2.

Axton Quarry (or Acton) 5½ m NW of Holywell, Flints., in Derbyshire-peak Limestone.
Spirifer oblatus, t 268!

Bagley-wood Pit, 2 m NE of Abingdon, Berks, in Coral Rag.
Turbo muricatus *e*, t 210, f 4.

Bajary Quarry, 4 m S of Thornhill, Dumfries-shire, in Derbyshire-peak Limestone.
Orthocera gigantea, t 246.

Bakewell W, Derby., see vol. 46, p. 213, 2 species, in Derbyshire-peak (1st) Limestone.
Spirifer striatus, t 270.

Banners-Ash, 1 m N of Wooton-Basset, Wilts, in Coral Rag.
Turbo muricatus α, t 210, f 4.

Barrow, 5 m W of Bury St. Emunds, Suff., in alluvial Clay, on [Chalk.
Ammonites biplex, t 293, f 1 and 2.

Barton, Cliff, Hants, see vol. 46, p. 213, and vol. 52, p. 351, 43 species, in London Clay.

Ampullaria acuta, t 284, *up*.
——— patula, t 284, *mid*.
——— sigaretina, t 284, *lo*.
Arca appendiculata, t 276, f 3.
——— Branderi, t 276, f 1 & 2.
Conus concinnus, t 302, f 2.
——— dormitor, t 301.
——— scabriculus α & β, t 303, f 2.
Corbula pisum, t 209, f 4.
——— revoluta α & β, t 209, f 8 to 13.
Fusus? bifasciatus t 228.
· bulbiformis α to δ, t 291, f 1 to 6.
· fisculneus α to γ, t 291, f. 7.

Murex

Black-Rock near Coik, Irel., see vol. 46, p. 214, and vol. 53, p. 352, 6 species, in Derbyshire-peak Limestone.

Spirifer pinguis, t 271.
——— striatus, t 270. [Clay.

Bognor-Rocks, Sussex, see vol. 46, p. 214, 5 species, in London
Ampullaria sigaretina, t 284, *lo*.

Bourton, 2½ m N of Banbury, Oxf., in Blue Marl on Lias.
Modiola Scalprum, t 248, f 2.

Bradford, E, Wilts, see vol. 46, p. 214, and vol. 52, p. 352, 2 species, in Clay on upper Oolite.
Avicula costata δ, t 244, f 1; see vol. 53, p. 122.

Ditto S in canal, in Fullers' Earth Strata.
Modiola gibbosa, t 211, f 2. [Oolite.

Ditto SW see vol. 52, p. 352, 1 species, in under

Bramerton, Norfolk, see vol. 46, p. 214, and vol. 52, p. 352, 10 species, in Crag Marl.
Cardium angustatum, t 283, f 2.
——— edulinum, t 283, f 3.

Braunston Tunnel, 2 m N of Daventry, Northam., in Blue Marl on Lias.
Plicatula spinosa, t 245, f 1. [Clay.

Brentford, Middlesex, see vol. 46, p. 214, 2 species, in London
Cardita margaritacea β, t 297, f 2.

Brokenhurst, Hants, see vol. 46, p. 215, and 52, p. 353, 2 species, in London Clay.
Fusus bulbiformis α to δ, t 291, f 1 to 6. | Melanea minima, t 241, f 3.
——— truncata, t 241, f 4.

Bromley, 4½ m S of Greenwich, Kent, in London deep-well Strata.
Ostrea pulchra, t 297.

Buxton, N, Derbyshire, see vol. 46, p. 215, 1 species, in Limestone Shale.

Ditto SE, see vol. 46, p. 215, 3 species, in Derbyshire-peak 3d Limestone.
Melanea constricta, t 218, f 2.

Ditto SW, see vol. 46, p. 215, 1 species, in Derbyshire-peak 4th Limestone.

Calne, 6 m N by W of Devizes, Wilts, in Coral Rag.
Lima rudis, t 214, f 1. | Pecten arcuatus β, t 205, f 5.

Ditto W, see vol. 52, p. 353, 1 species, in Clunch Clay.

Cambridge, Castle-hill, Cambridgeshire, see vol. 46, p. 215, 1 species, in Chalk Marl.
Inoceramus concentricus, t 305.
——— sulcatus α, t 306.

Ditto N, see vol. 52, p. 353, 1 species, in Oak-tree Clay.

Camp-

Campton, 2 m SW of Shefford, Beds., in Oak-tree Clay.
Inoceramus sulcatus β, t 306, f 6.
Castle-Combe, E, 5 m NW of Chippenham, Wilts, in Forest Marble.
Pecten rigidus, t 205, f 8.
Castleton, Derbyshire, see vol. 52, p. 353, 1 species, in Derbyshire-peak 1st Limestone.
Spirifer striatus, t 270.
Ditto W, see vol. 46, p. 215, 1 species, in Derbyshire-peak 3d Limestone.
Ditto W, see vol. 46, p. 215, 1 species, in Derbyshire-peak 4th Limestone.
Cave-Dale, N end, S part of Castleton, Derbyshire, in Derbyshire-peak 1st Limestone.
Spirifer trigonalis, t 265.
Charlton, in Kent, see vol. 52, p. 353, 5 species, in London deep-well Strata.
Ostrea tener, t 252, f 2 and 3.
——— ....? p. 141.
Charmouth Cliff, Dorsetshire, see vol. 46, p. 215, 1 species, in Blue Marl on Lias.
Ammonites Birchi, t 267.
——— Koenigi β, t 263, f 3.
Charterhouse-Hinton, E, 3½ m SSE of Bath, Somerset., in Clay on upper Oolite.
Avicula costata δ, t 244, f 1; see vol. 53, p. 122.
Chatley, Somersetshire, see vol. 52, p. 353, 2 species, in Kelloways stone.
Ditto see vol. 46, p. 215, and 52, p. 353, 5 species, in Cornbrash Limestone.
Pecten laminatus, t 205, f 4.
Chelmerton, 6 m W of Bakewell, Derby., in Derbyshire-peak Limestone.
Spirifer glaber, t 269, *up*.
Cheltenham, 7 m NW of Gloucester. in Blue Marl on Lias.
Trochus imbricatus, t 272, f 3 and 4.
Ditto W in upper Lias Limestone.
Hippopodium ponderosum α, t 250.
Cheriton, North, 2 m SW of Wincanton, Somerset., in Cornbrash Limestone.
Avicula echinata α, t 243, f 1.
Chicksgrove Quarry, Wiltshire, see vol. 52, 3 species, in Portland Rock: see the Strata, Min. Conch. II. p. 58.
Pecten lamellosus, t 239. [Rock.
Chilmark, Wiltshire, see vol. 52, p. 354, 1 species, in Portland
Nerita sinuosa, t 217, f 2.

Chippen-

Chippenham, 4 m W of Calne, Wilts, in Cornbrash Limestone.
Avicula echinata α, t 243, f 1. [Clay.
Christchurch, Hants, see vol. 46, p. 216, 1 species, in London
Ampullaria acuta, t 284, *up.* [Strata.
Claverton Hill, 1½ m E of Bath, Somerset., in Fullers' Earth
Modiola gibbosa, t 211, f 2.
Claydon, 5½ m N of Banbury, Oxfordsh., in under Oolite.
Mya V-scripta β, t 224, f 2 and 4.
Clophill, 4 m W of Shefford, Beds., in Oak-tree Clay.
Inoceramus sulcatus β, t 306, f 6.
Closeburn Quarry, 2½ m SSE of Thornhill, Dumfries-shire, Scot., in Derbyshire-peak Limestone; see the Strata, Min. Conch. III. p. 82.
Nautilus bilobatus, t 249, f 2 and 3.
——— pentagonus, t 249, f 1
——— tuberculatus, t 249, f 4.
Orthocera cordiformis, t 247.
——— gigantea. t 246.
An involute Shell ? t 249, f 5.
Closworth, 3½ m S of Yeovil, Somerset., in Cornbrash Limetone.
Avicula echinata α, t 243, f 1.
Colebrook-Dale, Shropshire, see vol. 52, p. 354, 1 species, in Coal-measures.
Ditto W, see vol. 46, p. 216, and 52, p. 354, 4 species, in Derbyshire-peak Limestone.
Hippopodium ponderosum β, t 250.
Cork, see *Black Rock.*
Cotswold-Hills, Gloucestershire, see vol. 46, p. 216, 1 species, in the upper Oolite.
Ditto see vol. 52, p. 354, 1 species in under Oolite.
Isocardia rostra, t 295, f 3.
Courtagnon, France, in Crag Marl.
Venericardia senilis, t 258. [Marl on Lias.
Craymouth ?, Dorsetshire, see vol. 46, p. 216, 1 species, in Blue
Ammonites Birchi, t 267.
Crick Tunnel, Northamptonshire, see vol. 46, p. 216, 1 species, in Blue Marl on Lias.
Plicatula spinosa ? t 245.
Cropredy, 4 m N of Banbury, Oxf., in under Oolite.
Ammonites annulatus β, t 222, f 1.
Unio concinnus, t 223, f 1 and 2.
Ditto in Sand under inferior Oolite.
Ammonites .. .. ? p. 143 | Helicina polita, t 285.
Cross-Hands, 2½ m E of Chipping-Sodbury, Gloucest., in under Oolite.
Cucullæa oblonga, t 206, f 2.
Derbyshire, County, see vol. 46, p. 216, 2 species, in Coal-measures.

Derbyshire,

Derbyshire, see vol. 46, p. 216, and 52, p. 354, 4 species, in Derbyshire-peak Limestone.
Spirifer oblatus, t 268.

Derry-hill, 3 m W of Calne, Wilts, in Coral Rag.
Turbo muricatus α, t 240, f 4. [Chalk.

Devizes, NE, Wilts, see vol. 46, p. 216, 1 species, in upper
Ditto N in Canal, see vol. 46, p. 216, and vol. 52, p. 354, 10 species, in Green Sand.
Pecten arcuatus α, t 205, f 7.

Draycot, 5 m S of Malmsbury, Wilts, in Cornbrash Limestone.
Avicula echinata α, t 243, f 1.

Dundry Hill, Somersetshire, see vol. 52, p. 355.
Ammonites Browni, t 263, f 4 and 5.
——— Sowerbii α & β, t 213.
Astarte excavata, t 233.
Cardita lunulata, t 232, f 1 & 2.
——— similis, t 232, f 3.
Cirrus Leachi, t 219, f 3.
——— nodosus, t 219, f 1, 2, and 4.
Cucullæa oblonga, t 206, f 1.
Melanea lineata, t 218, f 1.
Nerita lævigata, t 217, f 1.
Pecten barbatus, t 231.
Trigonia striata, t 237.
Trochus fasciatus, t 220, f 1.
——— granulatus, t 220, f 2.
——— ornatus, t 221, f 1.
——— sulcatus, t 220, f 3.
Turbo ornatus, t 240, f 1 and 2. [Oolite.

Dursley, Gloucestershire, see vol. 52, p. 355, 1 species, in under
Ditto in Blue Marl on Lias.
Avicula inequivalvis γ, t 241, f 2.

Emsworth, Hants, see vol. 52, p. 355, 1 species, in alluvial Sand.
Ditto S, in Harbour, in London Clay.
Ostrea flabellula, t 253, f 8.
Ditto N, see vol. 52, p. 355, 1 species, in upper Chalk.

Farley, 3½ m E of Salisbury, Wilts, in London deep-well Strata.
Ostrea undulata, t 238, f 2. [Strata.

Faversham, 8 m W by N of Canterbury, in London deep-well
Cucullæa decussata, t 206, f 3 and 4.

Felmersham, Bedfordshire, see vol. 46, p. 217, 1 species, in alluvial Clay.
Ditto SW see vol. 46, p. 217, 1 species (Terebratula obsoleta α), in Clunch Clay; see vol. 53, p. 130.
Mya? literata α, t 224, f 1. Limestone.
Ditto see vol. 46, p. 217, 3 species, in Cornbrash
Lutraria ovalis β, t 266, f 1.
Modiola aspera, t 212, f 4.
——— imbricata, t 212, f 1.
Modiola plicata, t 218, f 1.
Venus varicosa, t 296.

Fenny-Compton Tunnel, 5½ m S by E of Southam, Warw., in Lias Limestone.
Hippopodium ponderosum α, t 250, *lo.*

Folk-

Folkstone, N, Kent, see vol. 46, p. 217, and 52, p. 355, 15 sp., in Chalk Marl.

Hamites nodosus, t 216, f 3.
——— spiniger, t 216, f 2.
——— tuberculatus, t 216, f 4 and 5.
——— turgidus, t 216, f 6.
Inoceramus concentricus, t 305.
——— sulcatus α, t 306, f 1 to 4 and 7.
Trochus Gibbsi, t 278, f 1.

France, in London Clay.

Venericardia oblonga, t 289, *lo*.

Ditto, see vol. 52, p. 356, 1 species, in under Oolite.

Ditto, in Blue Marl on Lias.

Plicatula spinosa, t 245, f 3.

Frethern in Gloucestershire, see vol. 52, p. 356, 1 species, in Blue Lias.

Avicula inequivalvis β, t 244, f 2. | Lima antiquata, t 214, f 2.

Garsington, 4½ m S of Oxford, in Portland Rock.

Trochus reticulatus, t 272, f 2.

Gisleham, Suffolk, see vol. 46, p. 218, 2 species, in alluvial Stone.

Modiola imbricata, t 212, f 3.

Golleville, 5 m SSW of Volagnes, in Dep. of Channel, France, in (see Columby and Volagnes).

Venus? ? p. 174.

Grignon in France, see vol. 52, 2 species, in London Clay, (coarse Limestone).

Fusus ficulneus α to γ, t 291, f. 7.
Murex minax, t 229, f 2.
Ostrea flabellula, t 253.
Seraphs convolutus, t 286.

Gunton, Suffolk, see vol. 46, p. 218, 2 species, in Crag Marl.

Astarte planata, t 257. | Terebratula inconstans β, t 277, f 3.

Haldon-Hills, Devonshire, see vol. 46, p. 218, and 52, p. 356, 6 species, in Green Sand.

Terebratula dimidiata, t 277, f 5.
Trigonia affinis, t 208, f 3.

Harwich, SE, in Essex, see vol. 46, p. 218, and 52, p. 356, 3 species, in Crag Marl.

Venericardia senilis, t 258.

Havre de Grace, mouth of the Seine, in France, in London deep-well Strata.

Ammonites biarmatus, p. 122.

Hembury-Fort, 5 m NW of Honiton, Devon., in Green Sand.

Trigonia eccentrica, t 208, f 1 and 2.

Highgate Archway, Middlesex, see vol. 46, p. 218, and 52, p. 356, 30 species, in London Clay.

Ammonites decipiens, t 294.
Conus concinnus, t 302, f 2.
Conus concinnus major? t 302, f 1.

Corbula

Corbula globosa, t 209, f 3.
Fusus bifasciatus, t 228.
Modiola subcarinata? t 210, f 1.
Murex coronatus, t 230, f 3.
— cristatus, t 230, f 1 & 2.
Murex minax, t 229, f 2.
—— tuberosus, t 229, f 1.
Rostellaria macroptera α, t 30, 298, and 300.
Trochus extensus, t 278, f 3.

Hinton, see *Charterhouse H.*

Holywells, near Ipswich, Suff., see vol. 46, p. 219, and vol. 52, p. 357, 29 species, in Crag Marl.

Mytilus aliformis, t 275, f 4.

Hordwell Cliff (or Hordle), Hants, see vol. 46, p. 219, and vol. 52, p. 357, 9 species, in London Clay.

Corbula pisum, t 209, f 4.
Fusus acuminatus, t 274, f 1 to 3.
—— asper, t 274, f 4 to 7.
—— bulbiformis α to δ, t 291, f 1 to 6.
—— ficulneus α to γ, t 291, f 7.
Fusus rugosus, t 274, f 8 and 9.
Melanea costata, t 241, f 2.
Ostrea flabellula, t 253, f 1.
Rostellaria macroptera β, t 299.
Terebellum fusiforme α, t 287.
Venericardia globosa α & β, t 289, *up.* and *mid.*

Hot-Wells, ½ m W of Bristol, Glouc., in Derbyshire-peak Limestone.

Conularia quadrisulcata α, t 260, f 4 and 5.

Ilminster, S, Somerset., see vol. 52, p. 357, 1 species, in Clunch [Clay.
Ditto, see vol. 46, p. 220, and vol. 52, p. 357, 5 species, in under Oolite.

Ammonites annulatus β, t 222, f 3 to 5.
———— falcifer, t 254, f 2.
———— Strangewaysi, t 254, f 1 and 3.

Inver-Brora Colliery, 10 m NE of Dornoch, Sutherland, in Coal-measures.

Ammonites ? p. 176. | Cardita ? t 297, f 4.

Ipswich, 11 m SE of Stowmarket, Suff., in Crag Marl.

Cardium edulinum, t 283, f 3.
Mytilus antiquorum, t 275, f 1 to 3.

Ireland, Isle, in Derbyshire-peak Limestone.

Spirifer glaber, t 269, *mid.* | Terebratula Mantiæ, t 277, f 1.

Islington, G. J. Canal, Middlesex, see vol. 46, p. 221, 1 species, in London Clay.

Isocardia sulcata, t 295, f. 4.

Kelloways-Bridge, Wilts, see vol. 46, p. 220, and vol. 52, p. 357, 5 species, in Kelloways stone.

Ammonites Koenigi α, t 263, f 1 and 2.
Avicula inequivalvis α, t 244, f 3.
Isocardia tener, t 295, f 2.
Mya V-scripta α, 224, f 3.

Kelweston (or Kelston) SW, 3½ m W by N of Bath, Somerset., in Blue Lias.

Avicula inequivalvis β, t 244, f 2.

Langston-Herring, 5 m NW of Weymouth, Dorset., in Cornbrash Limestone.

Avicula echinata α, t 243, f 1. [Lias.

Leonard-Stanley, 4 m NE of Dursley, Glouc., in Blue Marl on

Plicatula spinosa, t 245.

Lewes, E, Sussex, see vol. 46, p. 220, and vol. 52, p. 358, 2 species, in upper Chalk?

Ditto, NW, see vol. 46, p. 220, 2 species, in lower Chalk?

Ditto, N, in Chalk Marl.

Inoceramus concentricus, t 305.

——— sulcatus α, t 306, f 5.

Little-Sodbury, Gloucestershire, see vol. 46, p. 220, and vol. 52, p. 358, 5 species, in under Oolite.

Cucullæa oblonga, t 206, f 1 and 2?

Trigonia duplicata, t 237, f 4 and 5. [Stone.

Little-Somerford, 5½ m W of Cricklade, Wilts., in Kelloways

Mya V-scripta α, t 224, f 3.

Llantrissent, S, Glamorganshire, see vol. 52, p. 358, 1 species, in Derbyshire-peak Limestone.

Modiola bipartita β, t 210, f 4.

Longleat Park, E, Wiltshire, see vol. 46, p. 220 (and Pecten quadricostata, t 56, f 1 and 2), 3 species, in Green Sand.

Ditto W, in Coral Rag.

Turbo (muricatus β and γ)? t 240, f 4. [Limestone.

Lullington, 2½ m NNE of Frome, Somerset., in Cornbrash

Avicula echinata α, t 243, f 1.

Lyme-Regis, Dorset., see vol. 46, p. 222, 2 species, in Marlstone.

Ditto NE, see vol. 52, p. 358, 6 species, in Blue Marl on Lias.

Ammonites Bechei α, t 280. Helicina expansa, t 273, f 1 to 3.
Birchi, t 267. solarioides, t 273, f 4.

Lyndhurst (Brick-kiln), 6½ m N of Lymington, Hants, in London Clay.

Ostrea flabellula, t 253, f 2 to 7 and 9. | Venericardia deltoidea, t 259, f 1. [stone.

Madagascar Isle, in Indian Ocean, in Cornbrash? Lime-

Isocardia minima, t 295, f 1.

Malling, 5 m W of Maidstone, Kent, in Chalk Marl.

Inoceramus concentricus, t 305.

——— sulcatus α, t 306.

Marcham (Field) Berkshire, see vol. 52, p. 358, 3 species, in Coral Rag (not Portland R.)

Trochus bicarinatus, t 221, f 2.

Milton-

Milton-Ernest, 4 m NNW of Bedford, in Cornbrash Limestone.
Modiola imbricata, t 212, f 1. [1st Limestone.
Monyash, 4 m WSW of Bakewell, Derbyshire, in Derbyshire-peak
Spirifer trigonalis, t 265, f 2 and 3. [don Clay.
Muddyford, Hampshire, see vol. 46, p. 221, 1 species, in Lon-
Conus dormitor, t 301.
Narford, 4 m NW of Swaffham, Norf., in alluvial Marl.
Lutraria ambigua, t 227.
New-Malton, 17 m NE of York, in Coral Rag Rock.
Ammonites trifidus, t 292, and 293, f 4.
Mya ? literata β, t 224, f 1.
Normandy Province (Dept. of Calvados, Channel, Eure and lower Seine) in France, in under Oolite.
Ostrea (fibrosa) ? p. 66. | Turbo ornatus, t 240, f 1 & 2.
North-Sands on the Shore near Scarborough, Yorkshire, in Coral Rag, lower part.
Mya ? literata β, t 244, f 1.
Norton, 3 m SW of Malmsbury, Wilts, in Cornbrash Limestone.
Avicula echinata α, t 243, f 1.
Norton Under-Hamdon ?, see *Wooton-Underedge.*
Osmington, S, Dorsetshire, see vol. 46, p. 221, 1 species, in Brick-Earth (not Blue Marl).
Modiola bipartita α, t 210, f 3; see vol. 53, p. 125.
Overton, ¼ m SSW of Ashover, Derbyshire, in Derbyshire-peak
Spirifer trigonalis, t 265, f 1. [1st Limestone.
Oxford, SE, in Portland Rock, lower Marl.
Terebratula inconstans α, t 277, f 4.
Ditto see vol. 46, p. 221, and vol. 52, p. 359, 2 species, in Oak-tree Clay.
Oxfordshire (Stonesfield ?) in Forest Marble.
Pecten Lens, t 205, f 2 and 3.
Pakefield Gravel-Pit, Suffolk, see vol. 46, p. 221, and vol. 52, p. 359, 3 species, in alluvial Stones.
Ammonites decipiens, t 294.
Parham-Park, S, 5½ m NNE of Arundel, Sussex, in Green Sand.
Trigonia aliformis, t 215, f 2.
Ditto in Brick-Earth.
Modiola æqualis, t 210, f 2.
——— bipartita α, t 210, f 3; see vol. 53, p. 125.
Paris, near, in France, see vol. 52, p. 359, 5 species, in London Clay (coarse Limestone ?).

Ampullaria acuta, t 284, *up.*
——— patula, t 284, *mid.*
——— sigaretina, t 284, *lo.*
Fusus bulbiformis α to δ, t 291, f 1 to 6.
Fusus rugosus, t 247, f 8 & 9.
Modiola subcarinata ? t 210, f 1.
Rostellaria macroptera α, t 298.
Terebellum fusiforme α, t 287.

Paris, near, in France, see vol. 52, p. 360, 2 species, in London deep-well Strata.

Pavingham, 5 m NW of Bedford, in Cornbrash Limestone.

Avicula echinata α, t 243, f 1.

Pegwell Bay, $2\frac{1}{2}$ m WSW of Ramsgate, Kent, in London deep-well Strata.

Cardita margaritacea α, t 297, f 1.

Pickeridge-Hill, Somersetshire, see vol. 46, p. 221, and vol. 52, p. 360, 5 species, in Blue Lias.

Modiola Hillana, t 212, f 2.

Plaistow, 3 m SW of Ilford, Essex, in London deep-well Strata.

Ostrea pulchra, t 279.

Portland Island (or Ferry), Dorsetshire, see vol. 46, p. 221, and vol. 52, p. 361, 4 species, in Portland Rock.

Lutraria ovalis α, t 226, f 2.
Pecten lamellosus, t 239.
Terebratula inconstans α, t 277, f 4.
Trochus reticulatus, t 272, f 2.

Ditto NE Coast, in Clunch Clay?

Ammonites Lamberti, t 242, f 1 to 3.
——— omphaloides, t 242, f 5.

Purbeck Peninsula, Dorsetshire, see vol. 52, p. 360, 1 species, in Portland Rock.

Ditto S, in Oak-tree Clay.

Ammonites rotundus, t 293, f 3.

Ramsgate, $4\frac{1}{2}$ m SSE of Margate, Kent, in upper Chalk.

Terebratula obliqua, t 277, f 2.

Ramshalt, $4\frac{3}{4}$ m SSE of Woodbridge, Suff., in Crag Marl.

Venus turgida, t 256, f 1.

Richmond-Park Well, see vol. 46, p. 222, and vol. 52, p. 360, 4 species, in London Clay.

Ditto (near bottom) in London deep-well Strata.

Cardita margaritacea α, t 297, f 1 to 3.

Ringmer, Sussex, see vol. 46, p. 222, and vol. 52, p. 360, 3 species, in Chalk Marl.

Venericardia? ? t 259, f 3.

Ringstead-Bay, 4 m NE of Weymouth, Dorset., in Portland R.

Terebratula inconstans α, t 277, f 4.
Trochus reticulatus, t 272, f 2.

Roydon-Green, Norfolk, see vol. 52, p. 361, 3 sp. in Crag Marl.

Astarte planata, t 257. | Venus turgida, t 256, f 2.

Sandfoot-Castle, near Weymouth, Dorset., see vol. 52, p. 361, 2 species, in Clunch Clay?

Ammonites Lamberti, t 242, f 1 to 3.
——— Leachi, t 242, f 4.
——— omphaloides, t 242, f 5.
Lima proboscidea, t 264.
Mytilus pectinatus, t 282.

Sand-

Sandown, 6½ m SE of Newport Isle of Wight, Hants, in Portland Rock, Sand.

Modiola aliformis, t 251.

Scalebar, Hill?, Yorkshire, see vol. 46, p. 222, 2 species, in Derbyshire-peak Limestone.

Spirifer obtusus, t 269, *lo*.

Scarborough, NNE, Yorkshire, see Min. Conch. II. p. 123, 1 species in Alum Shale.

Ditto (Castle), in Coral Rag Rock.

Mya? literata β, t 224, f 1. | Pinna lanceolata, t 281.

Scarlet-Head, 10 m SW of Douglas, Isle of Man, in Derbyshire-peak Limestone.

Ammonites Henslowi, t 262. | Nautilus complanatus, t 261.

Sheldon, 1½ m W of Chippenham, Wilts, in Cornbrash Limestone.

Avicula echinata α, t 243, f 1.

Sheppy Island, N Cliff, 15 m ENE of Rochester, Kent, in London Clay.

Trochus extensus, t 278, f 2. [Brick-Earth.

Shotover Hill, Oxfordshire, see vol. 46, p. 222, 1 species, in

Ditto see vol. 46, p. 222, and vol. 52, p. 361 (omitting Am. exca. and inserting Troc. ang. β, p. 95), 2 species, in Portland Rock.

Ditto see vol. 52, p. 361, 1 species, in Oak-tree Clay.

Ditto see vol. 52, p. 361 (and Am. exca. above) 2 species, in Coral Rag.

Ammonites trifidus, t 292, and 293, f 4.

Pecten similis, t 205, f 6.

Smallcomb, of Bath, Somerset., in Fullers'-Earth Strata.

Mya angulifera, t 224, f 6 and 7.

Somersetshire, in Portland Rock, Sand.

Ostrea Meadei, t 252, f 1 and 4.

Ditto in under Oolite.

Modiola cuneata, t 211, f 1.

Southill, 2 m NNE of Shefford, Beds., in Oak-tree Clay.

Inoceramus sulcatus β, t 306, f 6.

Steeple-Ashton, 2½ m E of Trowbridge, Wilts, in Coral Rag.

Turbo muricatus α, t 240, f 4. [Marble.

Stonesfield (or Stunsfield) 3 m W of Woodstock, Oxf., in Forest

Pecten obscurus, t 205, f 1. [stone.

Stoney-Stratford, 7 m ENE of Buckingham, in Cornbrash Lime-

Avicula costata γ, t 244, f 1; see vol. 53, p. 122.

——— echinata α, t 243, f 1.

Stubbington-Cliff, Hampshire, see vol. 46, p. 223, and vol. 52, p. 361, 18 species, in London Clay.

Pecten corneus, t 204. | Venericardia carinata, t 259, f 2.

Suffolk,

Suffolk, County, see vol. 52, p. 362, 6 species, in Crag Marl.
Trochus concavus, t 272, f 1. | Venericardia similis, t 258.
Ditto see vol. 52, p. 362, 1 species, in London deep-well Strata.
Ditto, NW, see vol. 46, p. 223, 1 species, in lower Chalk.

Swindon, 6½ m SSE of Cricklade, Wilts, in Portland Rock.
Pecten lamellosus, t 239.

Taunton, Somerset, see vol. 52, p. 362, 2 species, in Lias? (not Oolite; L. gibbosa γ).
Modiola minima, t 210, f 5.

Teignmouth (Little) Devon, see vol. 46, p. 223, 1 species, in Green Sand.
Trigonia pennata, t 237, f 6.

Tellesford, SW, 4½ m NNE of Frome, Somerset, in Cornbrash Limestone.
Avicula echinata α, t 243, f 1.

Thame, ½ m SE, 12½ m E of Oxford, in Portland Rock.
Pecten lamellosus, t 239.

Thornlie-Bank Quarry, 5 m ESE of Paisley, Renfrewshire, Scot., in Derbyshire-peak Limestone.
Orthocera cordiformis, t 247.

Tideswell, Derbyshire, see vol. 46, p. 223, 1 species, in Derbyshire-peak 3d Limestone.
Melanea constricta, t 218, f 2. | Spirifer glaber, t 269, *up*.
Ditto see vol. 46, p. 223, 1 species, in Derbyshire-peak 4th Limestone.

Tisbury, 2½ m SE of Hindon, Wilts, in Portland Rock.
Ostrea expansa, t 238, f 1.
Trigonia gibbosa α & β, t 235 and 236.

Toddenham, 4 m SE of Moreton in the Marsh, Glouc., in Blue Lias.
Hippopodium ponderosum α, t 250, *up*.

Tronlie-Bank, of Glasgow, Scotland, in Coal-measures.
Conularia quadrisulcata β, t 260, f 6.
——— ? teres, t 260, f 1 and 2.

Trowle, 1 m W of Trowbridge, Wilts, in Cornbrash Limestone.
Avicula echinata α, t 243, f 1. [on Lias.

Uley, near, 2 m W of Dursley, Gloucestershire, in Blue Marl
Plicatula spinosa, t 245, f 4.

Valognes, 11 m SE of Cherbourg, Dept. of Channel, in France, in (see Colomby, and Golleville).
Terebellum fusiforme β, t 287.

Westmoreland County (near Kendal?), in Derbyshire-peak Limestone.

Conu-

Conularia quadrisulcata *α*, t 260, f 3.
Spirifer oblatus, t 268.

Weymouth, see *Sandfoot-Castle.*

Whetstone-Pits, in Devonshire, see *Haldon Hills*: in Dorsetshire, see *Blackdown Hills.*

Whitby (Cliffs) Yorkshire, see vol. 46, p. 224, and vol. 52, p. 363, 7 species, in Alum Shale.
Ammonites annulatus *α*, t 222, f 2.
——— heterophyllus, t 266.

Wight, Isle of, Hants, in Cowes Rock, f. w.?
Melanea fasciata, t 241, f 1.

Ditto see vol. 52, p. 363, 1 species, in London deep-well Strata.

Ditto in Chalk Marl.
Hamites armatus (see t 168), t 234, f 2.

Wiltshire County, see vol. 52, p. 363, 1 species, in Chalk Marl.

Ditto see vol. 46, p. 224, and vol. 52, p. 363, 1 species, in Clunch Clay.

Ditto in Cornbrash Limestone.
Isocardia minima, t 295, f 1.

Wincanton, NW and SW, 14 m SSW of Frome, Somerset., in Cornbrash Limestone.
Avicula echinata *α*, t 243, f 1.

Winsley, 1½ m W of Bradford, Wilts, in Clay on upper Oolite.
Avicula costata *δ*, t 244, f 1; see vol. 53, p. 122.

Woodbridge, Suffolk, see vol. 46, p. 224, and vol. 52, p. 363, 5 species, in Crag Marl.
Cardium edulinum, t 283, f 3.
Mytilus antiquorum, t 275, f 1 and 3.
Venus turgida, t 256, f 2.

Wooton-Basset, 5 m W of Swindon, Wilts, in Coral Rag.
Turbo muricatus *α*, t 240, f 4.

Wooton Under-Edge? (query, Norton Under-Hamdon) 5½ m SE of Berkley, Gloucest., in under Oolite.
Lutraria lirata, t 225.

Yeovil, Somersetshire, see vol. 52, p. 363; Cirrus nodosus, vol. ii. p. 94, is redrawn and more fully described in the present volume, p. 35.

LXXI. *Some*

LXXI. *Some Memoranda respecting Caoutchouc.* By B. M. Forster, *Esq.*

To Dr. Tilloch.

Sir, — Since I mentioned to you, in my letter of 26th March, that I had expanded a small bottle of *Caoutchouc* (India-Rubber) by means of a condensing syringe, I have been informed of several instances of such bottles having been stretched in a like or nearly like manner some years ago, so that what I communicated to you as new, was not so.

I have some notion, that in one of the periodical publications a few years ago, there was an account of a bottle of this substance having been considerably enlarged by letting in coal gas, the bottle being attached to the end of a gas-light pipe: probably the *caoutchouc* might have been softened first.

An ingenious artist, well known by his excellent paintings for magic-lantern slides, named Matthias More, has informed me that he has stretched bottles of *caoutchouc* with a common pair of bellows, after they were become soft by having been soaked for many hours in warm (not boiling) water.

Mr. More also has stretched pieces of this substance to a very great length, until they became exceedingly thin and transparent. I have seen some small pieces or leaves of it, which appeared not unlike gold-beaters' skin, and were of a beautiful-looking substance. With such power, Mr. More says he has made air-balloons the size of a common hen's egg, which when filled with gas ascended. He had a plan for making long pieces of this substance, to use instead of glass slides for magic-lanterns, on which the figures were, I understand, to be *printed*, and afterwards coloured; but not succeeding in some part of the process, he gave up the scheme altogether. The pieces on which the figures were, he intended should be wound on, and off, an axis, in the manner that tapes for measuring are; so that, had the scheme succeeded, long processions might have been exhibited without the interruption occasioned by using several glass slides in frames, as usual.

May 4, 1822. B. M. Forster.

LXXII. *On two new Compounds of Chlorine and Carbon, and on a new Compound of Iodine, Carbon, and Hydrogen. By Mr.* FARADAY, *Chemical Assistant in the Royal Institution**.

ONE of the first circumstances that induced Sir H. Davy to doubt the compound nature of what was formerly called oxymuriatic acid gas, was the want of action of heated charcoal upon it; and considerable use of the same agent, and of the phænomena exhibited by it in different circumstances with chlorine, was afterwards made in establishing the simple nature of that body.

The true nature of chlorine being ascertained, it became of importance to form all the possible compounds of it with other elementary substances, and to examine them in the new view had of their nature. This investigation has been pursued with such success at different times, that very few elements remain uncombined with it; but with respect to carbon, the very circumstance which first tended to correct the erroneous opinions which, after Scheele's time, and before the year 1810, had gone abroad respecting its nature, proved an obstacle to the formation of its compounds; and up to the present time, the chlorides of carbon have escaped the researches of chemists.

That the difficulty met with in forming a compound of chlorine and carbon was probably not owing to any want or weakness of affinity between the two bodies, was pointed out by Sir H. Davy; who, reasoning on the triple compound of chlorine, carbon, and hydrogen, concluded that the attraction of the two bodies for each other was by no means feeble; and the discovery of phosgene gas by Dr. Davy, in which chlorine and carbon are combined with oxygen, was another circumstance strongly in favour of this opinion.

I was induced last summer to take up this subject, and have been so fortunate as to discover two chlorides of carbon, and a compound of iodine, carbon, and hydrogen, analogous in its nature to the triple compound of chlorine, carbon, and hydrogen, sometimes called chloric ether. I shall endeavour in the following pages to describe these substances, and give the experimental proofs of their nature.

If chlorine and olefiant gas be mixed together, it is well known that condensation takes place, and a colourless limpid volatile fluid is produced, containing chlorine, carbon, and hydrogen. If the volumes of the two gases are equal, the condensation is perfect. If the olefiant gas is in excess, that excess is left un-

* From the Transactions of the Royal Society for 1821. Read 21st of December 1820.

changed. But if the chlorine is in excess, the fluid becomes of a yellow tint, and acid fumes are produced. This circumstance alone proves that chlorine can take hydrogen from the fluid; and, on examination, I found it was without the liberation of any carbon or chlorine.

That the action thus begun, might be carried to its utmost extent, some of the pure fluid (chloric ether) was put into a retort with chlorine, and exposed to sunshine. At the first instant of contact between the chlorine and the fluid, the latter became yellow; but when in the sun's rays, a few moments sufficed to destroy the colour both of the fluid and the chlorine, heat being at the same time evolved. On opening the retort, there was no absorption, but it was found full of muriatic acid gas. This was expelled, and more chlorine introduced, and the whole again exposed to sun light: the colour again disappeared, and a few moist crystals were formed round the edge of the fluid. Chlorine being a third time introduced, and treated as before, it still removed more hydrogen; and now a sublimate of crystals lined the retort. Proceeding in this way until the chlorine exerted no further action, the fluid entirely disappeared, and the results were, the dry crystalline substance, and muriatic acid gas.

A portion of olefiant gas was then mixed in a retort with eight or nine times its bulk of chlorine, and exposed to sun light. At first the fluid formed; but this instantly disappeared; the retort became lined with crystals, and the colour of the chlorine very much diminished.

On examining these crystals, I found they were the compound I was in search of; but before I give the proofs of their nature, I will describe the process by which this chloride of carbon can be obtained pure.

Perchloride of Carbon,

A glass vessel was made in the form of an alembic head, but without the beak; the neck was considerably contracted, and had a brass cap with a stop-cock cemented on; at the top was a small aperture, into which a ground stopper fitted air tight. The capacity of the vessel was about 200 cubic inches. Being exhausted by the air-pump, it was nearly filled with chlorine; and being then placed over olefiant gas, and as much as could enter having passed in, the stop-cocks were shut, and the whole left for a short time. When the fluid compound of chlorine and olefiant gas had formed and condensed on the sides of the vessel, it was again placed over olefiant gas, and, in consequence of the condensation of a large portion of the gases, a considerable quantity more entered. This was left, as before, to combine with part of the remaining chlorine, to condense, and to form a partial

tial vacuum; which was again filled with olefiant gas, and the process repeated until all the chlorine had united to form the fluid, and the vessel remained full of olefiant gas. Chlorine was then admitted in repeated portions as before; consequently more of the fluid formed; and ultimately a large portion was obtained in the bottom of the vessel, and an atmosphere of chlorine above it. It was now exposed to sun light. The chlorine immediately disappeared, and the vessel became filled with muriatic acid gas. Having ascertained that water did not interfere with the action of the substances, a small portion was admitted into the vessel, which absorbed the muriatic acid gas, and then another atmosphere of chlorine was introduced. Again exposed to the light, this was partly combined with the carbon, and partly converted into muriatic acid gas; which being, as before, absorbed by the water, left space for more chlorine. Repeating this action, the fluid gradually became thick and opaque from the formation of crystals in it, which at last adhered to the sides of the glass as it was turned round; and ultimately the vessel only contained chlorine with the accumulated gaseous impurities of the successive portions, a strong solution of muriatic acid coloured blue from the solution of a little brass, and the solid substance.

I have frequently carried the process thus far in retorts; and it is evident that any conveniently formed glass vessel will answer the purpose. The admission of water during the process prevents the necessity of repeated exhaustion by the air-pump, which cannot be done without injury to the latter; but to have the full advantage of this part of the process, the gases should be as pure as possible, that no atmosphere foreign to the experiment may collect in the vessel.

In order to cleanse the substance, the remaining chlorine and muriatic acid were blown out of the vessel by a pair of bellows, introduced at the stoppered aperture, and the vessel afterwards filled with water, to wash away the muriatic acid and other soluble matters. Considerable care is then requisite in the further purification of the chloride. It retains water, muriatic acid, and a substance, which I find to be a triple compound of chlorine, carbon, and hydrogen, formed from the cement of the cap; and as all these contain hydrogen, a small quantity of any one remaining with the chloride would, in analysis, give erroneous results. Various methods of purification may be devised, founded on the properties of the substance, but I have found the following the most convenient:—The substance is to be washed from off the glass, and poured with the water into a jar; a little alcohol will remove the last portions which adhere to the glass; and this, when poured into the water, will precipitate the chloride, and the whole will fall to the bottom of the vessel. Then having

decanted the water, the chloride is to be collected on a filter, and dried as much as may be by pressure between folds of bibulous paper. It should next be introduced into a glass tube, and sublimed by a spirit-lamp: the pure substance with water will rise at first, but the last portions will be partially decomposed, muriatic acid will be liberated, and charcoal left. The sublimed portion is then to be dissolved in alcohol, and poured into a weak solution of potash, by which the substance is thrown down, and the muriatic acid neutralized and separated; then wash away the potash and muriate by repeated affusions of water, until the substance remains pure; collect it on a filter, and dry it, first between folds of paper, and afterwards by sulphuric acid in the exhausted receiver of the air-pump.

It will now appear as a white pulverulent substance; and if perfectly pure will not, when a little of it is sublimed in a tube, leave the slightest trace of carbon, or liberate any muriatic acid. A small portion of it dissolved in ether, should give no precipitate with nitrate of silver. If it be not quite pure, it must be resublimed, washed, and dried until it is pure.

This substance does not require the direct rays of the sun for its formation. Several tubes were filled with a mixture of one part of olefiant gas with five or six parts of chlorine, and placed over water in the light of a dull day; in two or three hours there was very considerable absorption, and crystals of the substance were deposited on the inside of the tubes. I have also often observed the formation of the crystals in retorts in common day light.

A retort being exhausted had 12 cubic inches of olefiant gas introduced, and 24·75 cubic inches of chlorine: as soon as the condensation occasioned by the formation of the fluid had taken place, 21·5 cubic inches more of chlorine were passed in, and the retort set aside in a dark place for two days. At the end of that time muriatic acid gas and the solid chloride had formed, but the greater part of the fluid remained unchanged. Hence it will form even in the dark by length of time.

I tried to produce the chloride by exposure of the two gases in tubes over water to strong lamp light for two or three hours, but could not succeed.

The perchloride of carbon, when pure, is immediately after fusion, or sublimation, a transparent colourless substance. It has scarcely any taste. Its odour is aromatic, and approaching to that of camphor. Its specific gravity is as nearly as possible 2. Its refractive power is high, being above that of flint glass (1·5767). It is very friable, easily breaking down under pressure; and when scratched has much of the feel and appearance of white sugar. It does not conduct electricity.

The

The crystals obtained by sublimation and from solutions of the substance in alcohol and ether, are dendritical, prismatic, or in plates; the varieties of form, which are very interesting, are easily ascertained, and result from a primitive octohedron.

It volatilizes slowly at common temperatures, and passes, in the manner of camphor, towards the light. If warmed, it rises more rapidly, and then forms fine crystals: when the temperature is further raised, it fuses at 320° Fahr. and boils at 360° under atmospheric pressure. When condensed again from these rapid sublimations, it concretes in the upper part of the tube or vessel containing it, in so transparent and colourless a state, that it is difficult, except from its high refractive power, to perceive where it is lodged, As the crust it forms becomes thicker, it splits, and cracks like sublimed camphor; and in a few minutes after it is cold, is white, and nearly opaque. If the heat be raised still higher, as when the substance is passed through a red hot tube, it is decomposed, chlorine is evolved, and another chloride of carbon, which condenses into a fluid, is obtained. This shall be described presently.

It is not readily combustible; when held in the flame of a spirit lamp, it burns with a red flame, emitting much smoke and acid fumes; but when removed from the lamp, combustion ceases. In the combustion that does take place in the lamp, the hydrogen of the alcohol, by combining with the chlorine of the compound, performs the most important part; nevertheless, when the substance is heated red in an atmosphere of pure oxygen, it sometimes burns with a brilliant light.

It is not soluble in water at common temperatures; or only in very small quantity. When a drop or two of the alcoholic solution is poured into a large quantity of water, it renders it turbid from the deposition of the substance. It does not appear that hot water dissolves more of it than cold water.

It dissolves in alcohol with facility, and in much greater quantity with heat than without. A saturated hot solution crystallizes as it cools, and the cold solution also gives crystals by spontaneous evaporation. When poured into water, the chloride is precipitated, and falls to the bottom in flakes. If burnt, the flame of the alcohol is brightened by the presence of the substance, and fumes of muriatic acid are liberated. Solution of nitrate of silver does not produce any turbidness in it, unless it be in such quantity that the water throws down the substance; but no chloride of silver is formed.

It is much more soluble in ether than in alcohol, and more so in hot than in cold ether. The hot solution deposits crystals as it cools; and the crystallization of a cold solution, when evaporated on a glass plate, is very beautiful. This solution is not precipitated

tated by water, unless the ether has previously been dried, and then water occasions a turbidness. Nitrate of silver does not precipitate it. When burned, muriatic acid fumes are liberated, but the greater part of the chloride remains in the capsule.

It is soluble in the volatile oils, and on evaporation is again obtained in crystals. It is also readily soluble in fixed oils. The solutions when heated liberate muriatic acid gas, and the oil becomes of a dark colour, as if charred.

Solutions of the acids and alkalies do not act with any energy on the substance. When boiled with solutions of pure potash and soda, it rises and condenses in the upper part of the vessel; and though it be brought down to the alkali many times, and reboiled, still the alkali, when examined, is not found to contain any chlorine, nor is any change produced. Ammonia in solution is also without action upon it. These solutions do not appear to dissolve more of it than pure water.

Muriatic acid in solution does not act at all upon it. Strong nitric acid boiled upon it dissolves a portion, but does not decompose it: as it cools, part of the chloride is deposited unaltered, and the concentrated acid, when diluted, lets more fall down. The diluted portion being filtered, and tested with nitrate of silver, gives no precipitate. It does not appear to be either soluble in, or acted upon by, concentrated sulphuric acid. It sinks slowly in the acid, and, when heated, is converted into vapour, which, rising through the acid, condenses in the upper part of the tube.

It is not acted upon by oxygen at temperatures under a red heat. A mixture of oxygen and the vapour of the substance would not inflame by a strong electric spark, though the temperature was raised by a spirit-lamp to about 400°. When oxygen mixed with the vapour of the substance is passed through a red-hot tube, there is decomposition; and mixtures of chlorine, carbonic oxide, carbonic acid, and phosgene gases are produced. A portion of the chloride was heated with peroxide of mercury in a glass tube over mercury; as soon as the oxide had given off oxygen, and the heat had risen so high as to soften the glass considerably, the vapour suddenly detonated with the oxygen with bright inflammation. The substances remaining were oxygen, carbonic acid, and calomel; and I believe there was no decomposition or action, until so much mercury had risen in vapour as to aid the oxygen by a kind of double affinity in decomposing the chloride of carbon.

Chlorine produces no change on the substance, either by exposure to light or heat.

When iodine is heated with it at low temperatures, the two substances melt and unite, and there is no further action. When heated

heated more strongly in vapour, the iodine separates chlorine, reducing the perchloride to the fluid protochloride of carbon, and chloriodine is produced. This dissolves, and if no excess of iodine be present, the whole remains fluid at common temperatures. When water is added, it generally liberates a little iodine; and on heating the solution, so as to drive off all free iodine, and testing by nitrate of silver, chloride and iodide of silver are obtained.

Hydrogen and the vapour of the substance would not inflame at the temperature of 400° Fahr. by strong electrical sparks; but when the mixture was sent through a red-hot tube, the chloride was decomposed, and muriatic acid gas and charcoal produced.

The vapour of the perchloride of carbon readily detonates by the electric spark with a mixture of oxygen and hydrogen gases; but the gaseous results are very mixed and uncertain, from the near equipoise of affinities that exist among the elements.

Sulphur readily unites to it when melted with it, and the mixture crystallizes on cooling into a yellowish mass. When heated more strongly, the substance rises unchanged, and leaves the sulphur unaltered; but when the mixed vapours are raised to a still higher temperature, chloride of sulphur and protochloride of carbon are formed. Sometimes there are appearances as if a carburet of sulphur were formed, but of this I have not satisfied myself.

Phosphorus at low temperatures melts and unites with the substance, without any decomposition. If heated in the vapour of the substance, but not too highly, it takes away chlorine, and forms the protochlorides of phosphorus and carbon. If heated more highly, it frequently inflames in the vapour with a brilliant combustion, and abundance of charcoal is deposited. Sometimes I have had the charcoal left in films stretching across the tubes, and occupying the space where the flame passed. The appearance is then very beautiful.

When phosphorus is heated with the vapour of the substance over mercury, so as not to inflame in it, there is generally a small portion of muriatic acid gas formed. If great care be taken, this is in very minute quantity; and its variable proportion sufficiently shows, that the hydrogen which forms it does not come from the substance. I am induced to believe that it is derived from moisture adhering to the phosphorus. The action of iodine on phosphorus shows, that it is very difficult to dry the latter substance perfectly.

A stick of phosphorus put into the alcoholic or ethereal solution of the perchloride did not exert any action upon it.

Charcoal heated in the vapour of the substance appears to have no action upon it.

Most

Most of the metals decompose it at high temperatures. Potassium burns brilliantly in the vapour, depositing charcoal, and forming chloride of potassium. Iron, zinc, tin, copper, and mercury, act on it at a red heat, forming chlorides of those metals, and depositing charcoal; and when the experiments are made with pure substances, and very carefully, no other results are obtained. Some of the substance was passed over iron turnings heated in a glass tube. At the commencement of the sublimation of the chloride through the hot iron, the common air of the vessels was expelled, and received in different tubes; but before one-third of the substance had been passed, all liberation of gas ceased, and the remainder was decomposed by the iron, without the production of any gaseous matters. The different portions of air that were thrown out being examined, the first proved to be common air, and the last carbonic oxide. This had resulted, probably, from the action of the chlorine on the lead of the glass tube. An evident action had taken place, and the oxygen evolved, meeting with the liberated carbon, would produce the carbonic oxide. This experiment has been repeated several times with the same results.

When the perchloride of carbon is heated with metallic oxides, different results are produced according to the proportions of oxygen in the oxides. The peroxides, as of mercury, copper, lead, and tin, produce chlorides of those metals, and carbonic acid; and the protoxides, as those of zinc, lead, &c. produce also chlorides; but the gaseous products are mixtures of carbonic acid and carbonic oxide. I have frequently perceived the smell of phosgene gas on passing the chloride over oxide of zinc; and as the substance easily liberates chlorine at high temperatures, it will be readily seen how a small portion of that gas may be formed. It also happens, sometimes, that the protoxides become blackened from the deposition of charcoal.

When the vapour of the chloride is passed over lime, baryta, or strontia, heated red hot, a very vivid combustion is produced. The oxygen and the chlorine change places, and both the metals and the carbon are burnt. Chlorides are produced, carbonic acid is formed and absorbed by the undecomposed parts of the earths, and carbon is deposited. In these experiments no carbonic oxide is produced. When passed over magnesia, there is no action on the earth, but the perchloride of carbon is converted by the heat into protochloride.

In these experiments with the oxides no trace of water could be perceived.

Having thus far described the properties of the substance, I shall now give the reasons which induce me to consider it a true chloride of carbon, and shall endeavour to assign its composition. My

My first object was to ascertain whether hydrogen existed in it or not. When phosphorus is heated in it, a small quantity of muriatic acid is generally formed; but doubt arises as to the cause of its production, from the circumstance that the phosphorus, as already mentioned, may be the source of the hydrogen. When potassium is heated in the vapour of the substance, there is generally a small expansion of volume, and inflammable gas produced; but it is very difficult to cleanse potassium both from naphtha and an adhering crust of moist potash; and either of these, though in extremely minute quantities, would give fallacious results.

A more unexceptionable experiment made with iron has been already described; and the inferences from it are against the presence of hydrogen in the compound.

Some of the substance in vapour was electrized over mercury by having many hundred sparks passed through it. Calomel was formed, and carbon deposited. A very minute bubble of gas was produced, but it was much too small to interfere with the conclusions drawn respecting the binary nature of the compound; and was probably caused by air that had adhered to the sides of the tube when the mercury was poured in.

The most perfect demonstration that the body contains no hydrogen, and indeed of its nature altogether, is obtained from the circumstances which attend its formation. When the fluid compound of chlorine and olefiant gas is acted on by chlorine and solar light in close vessels, although the whole of the chlorine disappears, yet there is no change of volume, its place being occupied by muriatic acid gas. Hence, as muriatic acid gas is known to consist of equal volumes of chlorine and hydrogen, combined without condensation, it is evident that half the chlorine introduced into the vessel has combined with the elements of the fluid, and liberated an equal volume of hydrogen; and as, when the chloride is perfectly formed, it condenses no muriatic acid gas, a method, apparently free from all fallacy, is thus afforded of ascertaining its nature.

I have made many experiments on given volumes of chlorine and olefiant gases. A clean dry retort was fitted with a cap and stop-cock. Its capacity was 25·25 cubic inches. Being exhausted by the air-pump, it was filled with nitrogen (24·25 cubic inches being required), and being again exhausted, 5 cubic inches of olefiant gas, and 10 cubic inches of chlorine, were introduced. It was then set aside for half an hour, that the fluid compound might form, and afterwards being placed again over a jar of chlorine, 19·25 cubic inches entered; so that the condensation had been as nearly as possible 10 cubic inches, or twice the volume of the olefiant gas (barometer 29·1 inches).

 It

It was now placed for the day (Oct. 18) in the rays of the sun; but the weather was not very fine. In the evening, the solid crystalline substance had formed in abundance, and very little fluid remained. When placed over chlorine, not the slightest change in volume had been produced. The stop-cock was now opened under mercury, and a small portion of the metal having entered, it was agitated in the retort, to absorb the chlorine; the neck of the retort was left open under the mercury all night, and the whole agitated from time to time. Next morning (barometer 29·6) the mercury which had entered, being passed into the neck of the retort, stood at a certain mark six inches above the level of the mercury in the trough, occupying 1·25 cubic inch, and leaving 24 cubic inches filled by the expanded muriatic acid gas and nitrogen. These volumes corrected to the pressure of 29·1 inches give 5·78 cubic inches for the chlorine absorbed, and 19·47 cubic inches for the muriatic acid gas, &c. These absorbed by water left 1·2 cubic inch of nitrogen; so that the gases in the retort, after the action of solar light, were,

	Cubic inches.
Muriatic acid gas	18·27
Chlorine	5·78
Nitrogen, &c.	1·2

and before that action,

Chlorine	29·25
Olefiant gas	5·0
Nitrogen	1·0

Hence 23·47 cubic inches of chlorine had disappeared, and 9·13 of these had entered into combination with an equal volume of 9·13 cubic inches of hydrogen liberated from the five cubic inches of olefiant gas, to form muriatic acid; and, consequently, 14·34 cubic inches of chlorine remained combined with the carbon of the five cubic inches of olefiant gas. Here, the volume of chlorine actually employed is not quite five times that of the olefiant gas, nor the volume of muriatic acid gas produced, equal to four times that of the olefiant gas; but they approximate; and when it is remembered that the conversion was not quite perfect, and that the gases used would inevitably contain a slight portion of impurity, the causes of the deficiency can easily be understood.

In other experiments made in the same way, but with smaller quantities, more accurate results were obtained: one cubic inch of olefiant gas with 12·25 cubic inches of chlorine, produced by the action of light 3·67 cubic inches of muriatic acid gas, 4·963 of the chlorine having been used. 1·4 cubic inch of olefiant gas with 12·5 cubic inches of chlorine produced 5·06 cubic inches of muriatic acid gas, 6·7 cubic inches of chlorine having been used.

used. Other experiments gave very nearly the same results; and I have deduced from them, that one volume of olefiant gas requires five volumes of chlorine for its conversion into muriatic acid and chloride of carbon; that four volumes of muriatic acid gas are formed; that three volumes of chlorine combine with the two volumes of carbon in the olefiant gas, to form the solid crystalline chloride; and that, when chlorine acts on the fluid compound of chlorine and olefiant gas, for every volume of chlorine that combines, an equal volume of hydrogen is separated.

I have endeavoured to verify these proportions by analytical experiments. The mode I adopted was, to send the substance in vapour over metals and metallic oxides at high temperatures. Considerable care is requisite in such experiments; for if the process be carried on quickly, a portion of fluid chloride of carbon is formed, and escapes decomposition. The following are two results from a number of experiments agreeing well with each other.

Five grains were passed over peroxide of copper in an iron tube, and the gas collected over mercury; it amounted to 3·9 cubic inches, barometer 29·85; thermometer 54° Fahr. Of these nearly 3·8 cubic inches were carbonic acid, and rather more than ·1 of a cubic inch was carbonic oxide. These are nearly equal to ·5004 of a grain of carbon. Hence, 100 of the chloride would give 10 of carbon nearly, but by calculation 100 should give 10·19. The difference is so small as to come within the limits of errors in experiment.

Five grains were passed over peroxide of copper in a tube made of green phial glass, and the chlorine estimated in the same manner as before. 17·7 grains of chloride of silver were obtained equal to 4·36 grs. of chlorine. This result approaches much nearer to the calculated result than the former; but there had still been action on the tube, and a minute portion of the substance had passed undecomposed, and condensed at the opposite end of the tube in crystals.

Experiments made by passing the perchloride over hot lime or barytes, promise to be more accurate and easy of performance. In the mean time, the above analytical results will, perhaps, be considered as strong corroboration of the opinion of the nature of the compound, deduced from the synthetical experiments; and the composition of the perchloride of carbon will be

Three proportions of chlorine	= 100·5
Two ditto carbon	= 11·4
	111·9

Protochloride of Carbon.

Having said so much on the nature of the perchloride of carbon,

bon, I shall have less occasion to dwell on the proofs that the compound I am about to describe, is also a binary combination of carbon and chlorine.

When the vapour of the perchloride of carbon is heated to dull redness, chlorine is liberated, and a new compound of that element and carbon is produced. This is readily shown by heating the bottom of a small glass tube, containing some of the perchloride in a spirit lamp. The substance at first sublimes; but as the vapour becomes heated below, it is gradually converted into protochloride, and chlorine is evolved.

It is not without considerable precaution that the protochloride of carbon can be obtained pure; for though passed through a great length of heated tube, part of the perchloride frequently escapes decomposition. The process I have adopted is the following: Some of the perchloride is introduced into the closed end of a tube, and the space above it, for 10 or 12 inches, filled with small fragments of rock crystal; the part of the tube beyond this is then bent up and down two or three times, so that the angles may form receivers for the new compound; then heating the tube and crystal to bright redness, and dipping the angles in water, the perchloride is slowly sublimed by a spirit lamp, and, on passing into the hot part of the tube, is decomposed; a fluid passes over, which is condensed in the angles of the tube, and chlorine is evolved; part of the gas escapes, but the greater portion is retained in solution by the fluid, and renders it yellow. Having proceeded thus far, by the careful application of a lamp and blow-pipe, the bent part of the tube may be separated from that within the furnace, and the end closed, so as to form a small retort; and on distilling the fluid four or five times from one angle to the other, all the chlorine may be driven off without any loss of the substance, and it becomes limpid and colourless. It still, however, always contains some perchloride, which has escaped decomposition; and, to separate this, I have boiled the fluid until the tube was nearly full of its vapour, and then closing the end that still remained open, by a lamp and blow-pipe, have afterwards left the whole to cool. It is then easy, by collecting all the fluid into one end of the tube, and introducing that end through a cork into a receiver, under which a very small flame is burning, to distil the whole of the fluid at a temperature very little above that of the atmosphere. The solid chloride being less volatile does not rise so soon, and the pure protochloride collects at the external end of the tube. To ascertain its purity, a drop may be placed on a glass plate; it will immediately evaporate, and if it contains perchloride, that substance will be left behind; otherwise, no trace will remain on the glass. The presence or absence of free chlorine may be ascertained

certained by dissolving a little of the fluid in alcohol or ether, and testing by nitrate of silver.

The pure protochloride of carbon is a highly limpid fluid, and perfectly colourless. Its specific gravity is 1·5526. It is a non-conductor of electricity. I am indebted to Dr. Wollaston for the determination of the refractive power of this chloride, and for the approximation to the refractive power given of the perchloride. In the present case it is 1·4875, being very nearly that of camphor. It is not combustible except when held in a flame, as of a spirit lamp, and then it burns with a bright yellow light, much smoke, and fumes of muriatic acid.

It does not become solid at the zero of Fahrenheit's scale. When its temperature is raised under the surface of water to between 160° and 170°, it is converted into vapour, and remains in that state until the temperature is lowered. When heated more highly, as by being passed over red-hot rock crystal in a glass tube, a small portion is always decomposed; nearly all the fluid may, however, be condensed again; but it passes slightly coloured, and the tube and crystal are blackened on the surface by charcoal. I am uncertain whether this decomposition ought not to be attributed rather to the action of the glass at this high temperature than to the heat alone.

It is not soluble in water, but remains at the bottom of it in drops, for many weeks, without any action.

It is soluble in alcohol and ether, and the solutions burn with a greenish flame, evolving fumes of muriatic acid.

It is soluble in the volatile and fixed oils. The volatile oils containing it burn with the emission of fumes of muriatic acid. When the solutions of it in the fixed oils are heated, they do not blacken or evolve fumes of muriatic acid. It is therefore probable, that when this happens with the solution of the perchloride in fixed oils, it is from its conversion by the heat into protochloride and the liberation of chlorine.

It is not soluble in alkaline solutions, nor do they act on it in some days. Neither is it at all soluble in, or affected by, strong nitric, muriatic, or sulphuric acids.

Solutions of silver do not act on it.

Oxygen decomposes it at high temperatures, forming carbonic oxide, or acid, and liberating chlorine.

Chlorine dissolves in it in considerable quantity, but has no further action, or only a very slow one, in common day light; on exposure to solar light, a different result takes place. I have only had two days, and those in the middle of November, on which I could expose the protochloride of carbon in atmospheres of chlorine to solar light; and hence the conversion of the whole of the protochloride was not perfect; but at the end of those two

two days the retorts containing the substances were lined with crystals, which, on examination under the microscope, proved to be quadrangular plates, resembling those of the perchloride of carbon. There were also some rhomboidal crystals here and there. After the formation of these crystals, there was considerable absorption in the retort; hence chlorine had combined; and the gas which remained was chlorine unmixed with any thing else, except a slight impurity. The solid body, on examination, was found to be volatile, soluble in alcohol, precipitable by water, and had the smell and other properties of perchloride of carbon. Hence, though heat in separating chlorine from the perchloride of carbon produces its decomposition, light occasions its reproduction.

It dissolves iodine very readily, and forms a brilliant red solution, similar in colour to that made by putting iodine into sulphuret of carbon, or chloric ether. It does not exert any further action on iodine at common temperatures.

An electric spark passed through a mixture of the vapour of the chloride with hydrogen, does not cause any detonation; but when a number are passed, the decomposition is gradually effected, and muriatic acid is formed. When hydrogen and the vapour of the protochloride are passed through a red-hot tube, there is a complete decomposition effected, muriatic acid gas being formed, and charcoal deposited. The mixed vapour and gas burn with flame as they arrive in the hot part of the tube. The vapour of the protochloride detonates readily by the electric spark with a mixture of oxygen and hydrogen gases, and a complete decomposition is effected. It will not detonate with the vapour of water.

Sulphur and phosphorus both dissolve in it, but exert no decomposing action at temperatures at, or below, the boiling point of the chloride. The hot solution of sulphur becomes a solid crystalline mass by cooling. Phosphorus decomposes it at a red heat.

Its action on metals is very similar to that of the perchloride. When passed over them at a red head, it forms chlorides, and liberates charcoal. Potassium does not act on it immediately at common temperatures; but, when heated in its vapour, burns brilliantly, and deposits charcoal.

When passed over heated metallic oxides, chlorides of the metals are formed, and carbonic oxide, or carbonic acid, according to the state of oxidation of the metal. When its vapour is transmitted over heated lime, baryta, or strontia, the same brilliant combustion is produced as with the perchloride.

While engaged in analysing this chloride of carbon, for the purpose of ascertaining the proportions of its elements, I endeavoured,

voured, at first, to find how much chlorine was liberated from a certain weight of perchloride during its conversion into protochloride, and for this purpose distilled the perchloride through red-hot tubes into solution of nitrate of silver, receiving the gas into tubes filled with and immersed in the same solution; but I could never get accurate results in this way, from the difficulty of producing a complete decomposition, and also from the formation of chloric acid. Five grains of perchloride distilled in this manner gave 4·3 grains of chloride of silver, which are equivalent to 1·06 grain of chlorine; but some of the chloride evidently passed undecomposed, and crystallized in the tube.

2·7 grains of the pure protochloride were passed over red-hot pure baryta in a glass tube: a very brilliant combustion with flame took place, chloride of barium and carbonic acid were produced, and a little charcoal deposited. When the tube was cold, the barytes was dissolved in nitric acid, and the chlorine precipitated by nitrate of silver. 9·4 grains of dry chloride of silver were obtained =2·32 grains of chlorine.

Other experiments were made with lime, which gave results very near to this, the quantity of chloride being rather less.

Three grains of pure protochloride were passed over peroxide of copper heated red-hot in an iron tube, and the gas received over mercury. 3·5 cubic inches of carbonic acid gas came over mixed with ·1 of a cubic inch of common air. These 3·5 cubic inches are nearly equal to ·449 of a grain of carbon.

These experiments indicate the composition of the fluid chloride of carbon to be one proportion of chlorine and one of carbon, or 33·5 of the former, and 5·7 of the latter. The difference between these theoretical numbers, and the results of the experiments, is not too great to have arisen from errors in working on such small quantities of the substance.

A mixture of equal volumes of oxygen and hydrogen was made, and two volumes of it detonated with the vapour of the protochloride in excess over mercury by the electric spark. The expansion was very nearly to four volumes; of these, two were muriatic acid, and the rest pure carbonic oxide: and calomel had been formed, its presence being ascertained by potash. Hence it appears, that one volume of hydrogen and half a volume of oxygen had decomposed one proportion of the protochloride, forming the two volumes of muriatic acid gas and one volume of carbonic oxide; and that at the intense temperature produced within the tube by the inflammation, the rest of the oxygen and the mercury had decomposed a further portion of the substance, giving rise to the second volume of the carbonic oxide, and to the calomel.

A mixture of two volumes of hydrogen and one volume of oxygen

oxygen was made, and three volumes of it detonated with the vapour, as before. After cooling, the expansion was to six volumes, four of which were muriatic acid, and two carbonic oxide. There was no action on the mercury in this experiment. Again, five volumes of the same mixture being detonated with the vapour of the substance, expanded to 9·75 volumes, of which 6·25 were absorbed by water and were muriatic acid, and 3·5 were carbonic oxide mixed with a very small portion of air introduced along with the fluid chloride. These experiments, I think, establish the composition of the protochloride of carbon, and prove that it contains one proportion of each of its elements.

From a consideration of the proportions of these two chlorides of carbon, it seems extremely probable that another may exist, composed of two proportions of chlorine combined with one of carbon. I have searched assiduously for such a compound, but am undecided respecting its production. When the fluid protochloride was exposed with chlorine to solar light, crystals were formed, as before described. The greater number of these were certainly the perchloride first mentioned in this paper; but when the retort was examined by a microscope, some rhomboidal crystals were observed here and there among those of the usual dendritic and square forms. These may, perhaps, be the real perchloride; but I had not time, before the season of bright sunshine passed away, to examine minutely what happens in these circumstances; and must defer this, with many other points, till the next year brings more favourable weather.

Compound of Iodine, Carbon, and Hydrogen.

The analogy which exists between chlorine and iodine naturally suggested the possible existence of an iodide of carbon, and the means which had succeeded with the one element offered the best promise of success with the other.

Iodine and olefiant gas were put in various proportions into retorts, and exposed to the sun's rays. After a while, colourless crystals formed in the vessels, and a partial vacuum was produced. The gas in the vessels being then examined, was found to contain no hydriodic acid, but only pure olefiant gas. Hence, the effect had been simply to produce a compound of the iodine with the olefiant gas.

The new body formed was obtained pure by introducing a solution of potash into the retort, which dissolved all the free iodine; the substance was then collected together and dried. It is a solid white crystalline body, having a sweet taste and aromatic smell. It sinks readily in sulphuric acid of specific gravity 1·85. It is friable; is not a conductor of electricity. When heated,

heated, it first fuses, and then sublimes without any change. Its vapour condenses into crystals, which are either prismatic, or in plates. On becoming solid after fusion, it also crystallizes in needles. The crystals are transparent. When highly heated it is decomposed, and iodine evolved. It is not readily combustible; but when held in the flame of a spirit lamp, burns, diminishing the flame, and giving off abundance of iodine, and some fumes of hydriodic acid. It is insoluble in water, or in acid and alkaline solutions. It is soluble in alcohol and ether, and may be obtained in crystals from these solutions. The alcoholic solution is of a very sweet taste, but leaves a peculiarly sharp biting sensation on the tongue.

Sulphuric acid does not dissolve it. When heated in the acid to between 300° and 400°, the compound is decomposed, apparently by the heat alone; and iodine and a gas, probably olefiant gas, are liberated. Solution of potash acts on it very slowly, even at the boiling point, but does gradually decompose it.

This substance is evidently analogous to the compound of olefiant gas and chlorine, and remarkably resembles it in the sweetness of its taste, though it differs from it in form, &c. It will with that body form a new class of compounds, and they will require names to distinguish them. The term chloric ether, applied to the compound of olefiant gas and chlorine, did not at any time convey a very definite idea, and the analogous name of iodic ether would evidently be very improper for a solid crystalline body heavier than sulphuric acid. Mr. Brande has suggested the names of hydriodide of carbon, and hydrochloride of carbon, for these two bodies. Perhaps, as their general properties range with those of the combustibles, while the specific nature of the compound is decided by the supporter of combustion which is in combination, the terms of hydrocarburet of chlorine, and hydrocarburet of iodine, may be considered as appropriate for them.

As yet I have not succeeded in procuring an iodide of carbon, but I intend to pursue these experiments in a brighter season of the year, and expect to obtain this compound.

LXXIII. *An Analysis of Mr.* Baily's *Astronomical Tables and Remarks for the Year* 1822. *By* George Harvey, *Member of the London Astronomical Society.*

To Dr. Tilloch.

Sir,—The celebrated astronomer Schumacher lately published at Copenhagen certain Astronomical Tables for the years 1820

1820 and 1821, and entitled *Astronomische Hülfstafeln*; and which have been considered by many astronomers as of the highest importance and value. In consequence however of their not reaching this country until more than half the current year for which they were intended had expired, Mr. BAILY, with a generous and disinterested zeal which merits the highest praise, resolved to prepare a set of similar tables, prior to the commencement of the present year, at his own expense, and to present copies of the same to his scientific friends. Mr. BAILY, it appears, only formed this resolution in September last, so that three months only remained, to collect, arrange, compute, and print such tables as appeared to him the best adapted to the general purposes of the practical astronomer. And with what success he has completed this important undertaking, every astronomer who possesses a copy, must have ample and satisfactory means of judging; and if I might be allowed to express my individual feelings on the subject, it would be that Mr. B. deserves the warmest and best thanks of the astronomical world.

It may however be possible, that some of the readers of your valuable Journal may not yet have had an opportunity of seeing Mr. BAILY's Tables; and I may, therefore, not be rendering an unacceptable service to them, if I endeavour to give a brief analysis of their contents.

The volume consists of three parts, the first of which presents a preface detailing the objects of the publication; the second contains explanations of the nature and uses of the tables; and the third is devoted to the tables themselves. It is an analysis of the two latter, which I intend to offer to your readers.

The first table contains a list of the principal occultations of fixed stars by the moon, visible at Florence, distinguishing the day, the name or number of the star, its magnitude, the catalogue from which it is taken, its right ascension and declination, and the time of its immersion and emersion. This table Mr. BAILY obtained from Baron ZACH's *Correspondance Astronomique*; and although, from its being calculated for the meridian and parallel of Florence, it is not adapted to Greenwich, it will be found, even in its present form, of much service to the practical astronomer, and will moreover convince him of the information he would derive from a table calculated for the latter place. It contains nearly 250 occultations, and all the small stars have been rejected, excepting such as take place within a few days of a new moon. The table occupies nine pages.

The second table is more general in its nature than the former, and may be regarded as a kind of supplement to it. It contains a list of all the stars, from the Catalogue of PIAZZI, near which the moon will pass, in her several lunations, during the

the present year; and which of course may exhibit an occultation in some part of the world. It contains the same number of pages as the preceding.

During the present and several of the following years, the cluster of stars called the Pleiades will present some singular facilities to the practical astronomer, on account of the moon's nodes being so situated, that she will pass over this beautiful cluster every lunation; and hence Mr. BAILY has introduced JEAURUT's Catalogue of the 64 stars which compose it, into his third table, reduced to the first day of the present year. This catalogue contains the synonyms, the last mentioned astronomer's number and magnitudes of the different stars, their right ascensions in time and degrees, and their declinations. The phænomenon above alluded to, will afford a very favourable opportunity for enabling astronomers to illustrate the method proposed by CAGNOLI, for determining the figure of the Earth, by means of occultations of the fixed stars by the Moon *. This table is accompanied by a chart, exhibiting the several positions of the stars, with their comparative magnitudes. This beautiful cluster has at all times attracted the attention of astronomers. KEPLER gave a chart of them in 1653; LA HIRE in 1693; CASSINI and MIRALDI in 1708, and OUTHIER in 1770.

The fourth table occupies 17 pages, and will be found very useful. It contains the mean places of all the stars visible in this latitude, above the 5th magnitude, with their annual variations, deduced from the observations of BRADLEY and PIAZZI, agreeably to the formula given by BESSEL in his *Fundamenta Astronomiæ*.

The fifth table contains all those stars inserted in the preceding table, within 30° of the equator, arranged in the order of their declinations. It occupies six pages.

The sixth table is devoted to the mean places of 36 principal stars for January 1, 1822, being those which are more particularly recorded in observatories. Their right ascensions are recorded, both according to M. BESSEL and Mr. POND.

The seventh table contains the apparent places of the stars recorded in the preceding table, for every 10th day of the year; and from the *differences* being annexed, the value for any intermediate day may be readily deduced. This table has been

* In the year 1819 Mr. BAILY, with the same liberal spirit as led to the publication of the present tables, printed for gratuitous circulation, the able and interesting Memoir of CAGNOLI on the Figure of the Earth. This Memoir appeared originally in the Transactions of the Italian Society (*Memorie di Matematica e di Fisica della Società Italiana*, Tom. vi. Verona, 1792) Although printed so many years ago, it does not appear to have attracted much attention, until the appearance of Mr. BAILY's translation.

 computed

computed from the tables recently published by M. Bessel in the fifth part of his *Astronomische Beobachtungen*, published at Konigsberg in 1820. Mr. Baily properly observes with respect to this table, "that it is difficult to account for the general consent which seems to have existed amongst astronomers, to observe more particularly those 36 stars, and which have thus acquired the name of *fundamental stars;* because they by no means furnish the best arrangement that might be made; some others might have been selected, more generally distributed over the heavens." Both M. Bessel, and our own Astronomer Royal, now make daily observations on every star above the fifth magnitude, a practice which immortalized the names of Bradley and Piazzi.

Table the eighth contains four pages devoted to the apparent place of the pole star, for every day of the year, at the time of its upper culmination. This table was computed by Dr. Struve, the director of the Observatory at Dorpat in Livonia, from the tables of M. Bessel before alluded to.

The ninth table contains a comparison of the mean right ascensions of the 36 principal stars given in the sixth table. Of the five columns which compose this table, the first contains the latest observations of Dr. Maskelyne, with the old transit instrument, and the remaining columns contain the results of Mr. Pond's observations, with the new transit instrument. Dr. Maskelyne's observations correspond with those of Mr. Pond, for the year 1816, with singular accuracy; but those for 1817, 1818, and 1819 present some remarkable variations.

The tenth table contains a comparison of the mean north polar distances of 34 principal stars, on January 1, 1822, as deduced from the observations of the Astronomer Royal, with the mural circle, during the years 1812, 13, 14, 15, 16, 19, and 20. Columns are also added, to exhibit the differences between these different years.

In the eleventh table will be found a list of all the eclipses of Jupiter's satellites visible at Greenwich. This table has been deduced by Mr. Baily, from the *Connaissance des Tems* for 1822, by allowing for the difference of the meridians. The computers of the latter work deduced the same from the tables of Delambre, published in 1817. There is a column also in this table, to denote the distance of the satellite from Jupiter's limb, at the moment of its reappearance, in terms of the planet's diameter, which is regarded as unity; the distance being measured, either in a line with the planet's equator, or in a line parallel thereto.

In the twelfth table, the apparent obliquity of the ecliptic and the equation of the equinoxes are given, for the first day of every month

month, and deduced from the observations of M. Bessel: a noble proof of the unceasing labour of the Konigsberg astronomer.

The thirteenth table will be regarded as of a novel but useful kind. It contains an ephemeris of the comet which is expected to return in the present year, calculated by M. Encke. It is calculated on two different hypotheses. According to the first, the passage of the perihelion will be on the 24th of May; and the second, the 25th. One part of the table is devoted to the positions of the comet, *before* the passage of the perihelion, and the other to its positions *after*.

Mr. Baily, when speaking in his preface of the activity of the continental astronomers, makes an eloquent remark, which may not be improperly introduced in this place. "If the appearance of a comet," says he, "is announced on the continent, not only is its course diligently watched, but in a few days its elements are computed, perhaps by several persons; and its orbit determined, and reserved for future comparisons. Whilst in this country, it is viewed with silent admiration; and its path vanishes equally from our sight, and our remembrance."

The fourteenth table contains an ephemeris of Venus for several days before and after her inferior conjunction, on the 10th of March; and has been adopted by Mr. Baily, from Schumacher's Ephemeris for 1821.

The remaining five tables contain similar ephemerides for the oppositions of Mars, Jupiter, Saturn, Uranus, and Ceres.

Some idea may now be formed of the nature of the tables under consideration. Mr. Baily has set an example, which, I devoutly hope, will not (to adopt the concluding but expressive words of his preface) "silently expire." It would be difficult to estimate the debt of gratitude which the practical astronomers of this country owe to this excellent astronomer, for his unceasing efforts to promote the advancement of the noblest of the sciences. In this country, there are many men of sterling genius, with minds well adapted to astronomical pursuits, who allow their fine and noble powers to run wild amidst trivial and unimportant pursuits, unconscious that they possess the means of cultivating the practical departments of this science with ability and success. On the other hand, in the lonely solitude of a village, it sometimes happens, that there exists a mind gifted with the energies necessary for the prosecution of those lofty pursuits, and who languishes with regret, conscious of his "power," but without the means of developing its force. But when such individuals read in the preface to these tables, the account which Mr. Baily has given of the observatory, if it may be so called, of the celebrated Olbers, of the man who has added two planets to the glittering triumphs of modern science;—they will no longer allow

low their powers to waste their efforts on the toys of science, or consume them with unavailing regrets. "An ordinary room," says Mr. B. "is the observatory of this illustrious astronomer." In this room, "he has no instrument fixed in the meridian;" and what is more remarkable, "it is impossible from its nature to do so." "Four instruments constitute the whole of his apparatus; namely, a telescope by DOLLOND, an equatorial telescope by REICHENBACH, a clock made at Bremen, and a small sextant with artificial horizon." The eclipse of a star by the cross of a neighbouring tower enables him to obtain his time, and which he corrects, by taking altitudes of some known star with his little sextant." In his observatory, the traveller from whose work Mr. B. extracted these facts, saw the SMALL telescope by which this illustrious man discovered Ceres and Pallas;—"yet with such slender means," continues Mr. BAILY, "how valuable have been the services which OLBERS has rendered to astronomy!"

Lord BACON has well delineated in his *Novum Organum*, in strong and figurative language, the influence which "Idols" have exercised on the progress and improvement of mankind. And in no science, perhaps, has this baneful and improper influence been more powerfully displayed than in Astronomy,—both in its earlier history and in its riper fruits. How many of our popular errors may not be traced to an astronomical source! And even in the present day, is there not a feeling sometimes entertained, "that in order to make any observations that can be essentially serviceable to the science, a large and splendid establishment is necessary?" "Nothing however," continues Mr. BAILY, "is more contrary to the fact." "The *fundamental* points of astronomy do indeed more properly belong to the public observatories, where the best instruments and best observers are generally to be found. But there are many other points of a *comparative* nature (an attention to which would only distract the public observer) which may be safely left to those private persons who have instruments adapted to such particular purposes."

Dr. KITCHINER, in a little book which ought to be in the hands of every young astronomer, most truly observes, that "all arts and sciences are more or less encumbered with errors and prejudices; and that astronomy is not free from these." The principal prejudice, or, to adopt the expressive term of Lord BACON, the "idol," which has confined the study of the minutiæ of astronomy to the observatories of the State, and of a few opulent individuals, is (the belief) that an immense apparatus of unwieldy magnitude, extremely costly to purchase, difficult to procure, and troublesome to use, is indispensably necessary to discern what has been described by various astronomers." "I

hope,"

hope," continues Dr. K. "I shall succeed in my endeavours to extinguish this vulgar error, and be able to prove, that neither such enormous instruments, nor monstrous magnifying powers, are either necessarily required, or commonly used, and thereby the contemplation of the wonderful and beautiful celestial bodies may become more *general*, the science *simplified* and made *easy*, and the study of it rendered universally attractive, and no longer *confined* to the happy few, whose good fortunes will furnish them with such expensive instruments; and I hope I shall clearly convince the amateurs of astronomy, that all the *principal* and *most interesting phænomena* are visible with glasses which are *easy to procure*, and *handy to use*; and that the rationale of telescopes has this in common with other sciences, that what is most worth learning is easiest learned, and is, like all other sciences, reduced to a few clear points."

"Most of the modern discoveries in astronomy," continues Dr. K. "have been made by Dr. HERSCHEL; these have not arisen from the wonderful magnitude of his optical machines, but from his *indefatigable and matchless perseverance as an observer*. Dr. H.'s first catalogue of double stars was made with a Newtonian telescope, of not quite seven feet focus, and with only four inches and a half aperture, charged with a power of 222."

Nothing that I can add, could increase the evidence which these unquestionable facts so decisively establish. That astronomy should be more generally, and at the same time more practically cultivated by the humble labourers in science, in this country, no one, however lofty may be his scientific pretensions, will for a moment venture to deny. The true method however to encourage the development of talent of this kind, is to destroy the "idols" which now cling to the roots and branches of the science;—to prove, that even the most eminent observers have not employed instruments and means much beyond the efforts of those who move in lower spheres; and that there remains a wide and fertile field for diligent research, open to the ardent labours and inquiries of those whose still feebler means prevent them from attempting those observations which form the ground-work and key-stones of the science.

LXXIV. *On the best Kind of Steel and Form for a Compass-Needle.* By Captain HENRY KATER, F.R.S.*

ON the return of the first expedition which sailed for the discovery of a North-west passage, it appeared, that from the near approach to the magnetic pole, and the consequent diminution of

* From the Transactions of the Royal Society for 1821, Part I.

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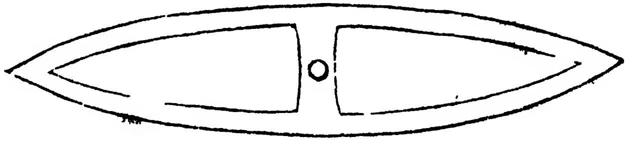
the directive force, the compasses on board had become nearly useless. Some of the azimuth compasses employed on that occasion were of my own invention; I was therefore anxious that the next expedition, which was about to sail under the command of Lieut. Parry, and which has happily returned with so much honour to those engaged in it, should be furnished with instruments of this description, combining as much power and sensibility as possible.

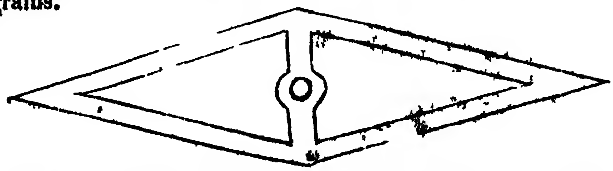
It was with this intention alone that I commenced the experiments which form the subject of the present paper; but which I should not have deemed sufficiently important to be made public, had I not lately, on resuming the inquiry, been led to some results which appeared of sufficient interest, as well as practical utility, to induce me to lay them before the Royal Society.

My immediate object was to ascertain the kind of steel and form of needle best calculated to receive the greatest directive energy with the least weight.

Two needles were prepared of that kind of steel which is called blister-steel, and two of spur-steel, the weight of each being sixty-six grains. They were of the form of a long ellipse, in length five inches, and in width half an inch. One of each kind was pierced, as in the figure below, the weight being made up by additional thickness. This needle, therefore, had much less extent of surface than the solid ellipse.



Recollecting to have had in my possession, many years since, a compass of extraordinary power, the needle of which was composed of pieces of steel-wire put together in the shape of a rhombus, I caused two needles to be made of this form of a piece of clock-spring, which I understand is of that kind of steel which is called shear-steel. They were shaped as below; in one, the cross piece was of brass, and in the other formed of part of the clock-spring. These needles were, by mistake, made to weigh only 45 grains.



In ascertaining the directive force, the balance of torsion of M. Coulomb

M. Coulomb was employed. This instrument, as is well known, consists of a fine wire, attached to an index, moveable round a circle divided into degrees. To the other end of the wire is fixed a cradle, to receive the needle which is the subject of experiment. The needle being in the magnetic meridian when the wire has no torsion, is afterwards forced to deviate from it to a mark distant about 60°, by turning the index, and consequently twisting the wire. The number of degrees passed over by the index will be as the directive force of the needle.

The needles which I have described were first made soft, and then hardened merely at their ends; they were not polished, and were magnetised to saturation.

Experiment 1.

Needles soft, and then hardened at the ends.	Weight of needle.	Directive force.
Blister-steel, solid ellipse	66	500
———— open ellipse	66	520
Spur-steel, solid ellipse	66	540
———— open ellipse	66	500
Shear-steel, rhombus	45	435
———— rhombus, with cross pieces of brass	45	435

By the experiments on magnetism made by M. Coulomb, it appears, that the directive forces of needles of similar form are to each other as their masses; the directive force, therefore, of a needle of the form of a pierced rhombus of 66 grains would be expressed, according to the preceding experiments, by 638.

From many other experiments, which I regret were not registered at the time, it appeared that shear-steel was capable of receiving the greater magnetic force, and that the pierced rhombus was the best form for a compass-needle. I may add, that needles of cast-steel were tried, but were found so very inferior as to be at once rejected.

My next object was to determine the effect of polish, and of various modes of hardening and tempering the needles. In addition to the former needles, two were made of *clock-spring* of the pierced rhombus form five inches long, two inches wide, and weighing 66 grains. One of these was first softened, then hardened at the ends, and left unpolished; the other, as well as the solid elliptical needle of spur-steel, was hardened throughout, and polished. The needles were then magnetised to saturation.

Experiment 2.

	Directive force.
Unpolished rhombus, hard at the ends	800
Polished rhombus, hard throughout	367
Polished elliptical-needle, hard throughout	380

	Directive force.
Polished elliptical-needle, softened in the middle by laying it on a red-hot poker	610
Polished rhombus, softened in the middle in the same manner	590

The needles were now laid aside till the following day, when the directive force was again examined.

Unpolished rhombus, hard at the ends	805
Polished elliptical-needle, softened in the middle ..	625
Polished rhombus, softened in the middle	580

The polished rhombus was now softened throughout; and the extremities being hardened at a red-heat, the directive force was found to be 800. It is scarcely necessary to say, that the needles were re-magnetised to saturation previous to each experiment.

From these experiments I drew the following conclusions:

That of the steel I employed, shear-steel is the best kind for compass-needles.

That the best form for a compass-needle is that of a pierced rhombus.

That polish has no influence on the directive force.

That hardening the needle throughout, considerably diminishes its capacity for magnetism.

That a needle soft in the middle, and its extremities hardened at a red-heat, appears to be susceptible of the greatest directive force.

That the directive force does not depend on the extent of surface, but on the mass.

I might also have inferred, that the needle was capable of a greater directive force when wholly softened and hardened at the extremities, than when entirely hardened and softened in the middle; but it will appear by subsequent experiments, to be detailed, that the difference is probably to be attributed to a difference in the degree of heat to which the needle is exposed in softening it in the middle.

My next experiments were made with three needles, two of which were rectangular parallelograms of equal length and weight, but the one only half the width of the other. The third needle was a pierced rhombus; the whole were made of clock-spring. These needles were made perfectly hard, and magnetised, as was always the case, to saturation.

Experiment 3.

Needles perfectly hard.	Directive force.
Wide parallelogram	490
Narrow parallelogram	490
Pierced rhombus	532

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An accident happened to these needles, which rendered them unfit for further experiment. It, however, appears from that above stated, that the directive force is nearly as the mass, and not as the surface; and that the pierced rhombus is superior to the parallelogram.

M. Coulomb having found that a needle of the rhombus form not pierced, and which he calls *une lame taillée en flèche*, was susceptible of a greater directive force than a parallelogram, I was desirous of repeating this experiment, as well as of comparing this form with the pierced rhombus. For this purpose four needles were made four inches and a half long, each weighing 63 grains; one was a parallelogram, 0·44 inch wide; another a rhombus, which I shall call the large rhombus, 0·8 wide; the third a pierced rhombus, 1·4 wide in the middle, having its sides 0·2 wide: these were made of clock-spring. The fourth needle a rhombus, which may be called the small rhombus, 0·4 wide, was made of that kind of steel which is used for saw-blades, and which I believe is shear-steel. This last needle was much thicker than the others.

The steel of which these needles were made had been exposed to a sufficient degree of heat to render it soft enough to be worked, and in this state the needles were magnetised.

Experiment 4.

Steel soft as worked.	Directive force.
Parallelogram	720
Small rhombus	530
Large rhombus	765
Pierced rhombus	813

Experiment 5.

The ends of the needles hardened at an obscure red-heat.	Directive force.
Parallelogram	715
Small rhombus	577
Large rhombus	790
Pierced rhombus	840

Experiment 6.

The ends hardened at a red-heat.	Directive force.
Parallelogram	742
Small rhombus	583
Large rhombus	745
Pierced rhombus	844

Experiment 7.

Hardened with a bright red-heat, and then softened by a red-heat from the middle toward the ends, the extremities for about an inch remaining hard.	Directive force.
Parallelogram	611
Small rhombus	710
Large rhombus	660
Pierched rhombus	685

Experiment 8.

Softened at a red-heat between two plates of steel, the whole being allowed to cool gradually, and then the extremities of the needles hardened at a red-heat.	Directive force.
Parallelogram	520
Small rhombus	585
Large rhombus	554
Pierced rhombus	590

As it appeared from the above experiments that the needles had suffered a gradual deterioration, I imagined that this might have occurred in consequence of their having been exposed to the heat of a coal fire, by which some portion of the carbon of the steel might have been destroyed: I therefore re-carbonised the needles, by surrounding them with shreds of leather, and exposing them for several hours, in a close vessel, to a considerable heat. After they had gradually cooled, the ends were hardened at a red heat.

Experiment 9.

Needles soft, and then the ends hardened at a red heat.	Directive force.
Small rhombus	477
Large rhombus	415
Pierced rhombus	450

Here we may remark, that though the needles were apparently in the same state, except being re-carbonised, as in the last experiment, they had suffered considerable deterioration.

The needles were now covered with a mixture, well known to workmen, to prevent decarbonisation: this had been before neglected, but was used in all the subsequent experiments. They were hardened at a bright red heat, and afterwards tempered throughout, rather beyond a blue colour. The large rhombus and the parallelogram were accidentally broken.

Experiment 10.

Hardened at a bright red, and then tempered beyond a blue.	Directive force.
Small rhombus	660
Pierced rhombus	577

From these last experiments I believe little can be gathered, except

except that the needles became less susceptible of directive force from repeated exposure to heat, and that this effect was not occasioned by a decarbonisation of the steel. The small rhombus of saw-blade, perhaps from being the thickest, suffered less than those made of clock-spring.

The springs of clocks are made by passing the steel between rollers; and it thus undergoes great compression. May not this state be favourable to magnetism; and the repeated expansion of the steel by heat, destroying this state, have occasioned the deterioration I have remarked?

The needle which was made of saw-blade having suffered less than the others in the preceding experiments, I procured three other needles of this material; they were cut out of the same plate; the weight of each was 120 grains, and their length four inches and a half. One was a parallelogram, 0·46 inch-wide; another a rhombus, as before, 0·87 inch-wide; and the third a pierced rhombus, having the middle 1·5 inch, and its sides 0·25 wide.

These needles were made without its being found necessary to soften the steel plate; they consequently were all as nearly as possible of the same degree of temper. In this state they were magnetised.

Experiment 11.

Steel the same as worked.	Directive force.
Parallelogram	1143
Rhombus	1020
Pierced rhombus	1085

Wishing to try whether the needles were magnetised to saturation, I carefully re-magnetised them.

Experiment 12.

Needles re-magnetised.	Directive force.
Parallelogram	1140
Rhombus	955
Pierced rhombus	1069

I now to my surprise found that the directive force, instead of increasing, had lessened in each of the needles, and I became anxious to discover the cause of so unexpected a phænomenon. It has been observed by M. Coulomb, and more fully entered into by Biot, that if a needle be magnetised to saturation by strong magnets, and afterwards weaker magnets be applied, the needle will lose some part of the force it had before acquired. Now, if in using the same set of magnets a certain degree of force be communicated to a needle, and the magnets be afterwards arranged in a manner less favourable for imparting magnetic force, it should seem that this second operation would produce the same effect as would follow the use of magnets of

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less force, and that the magnetism of the needle would suffer a diminution.

The method I had employed in magnetising the needles, was that of Du Hamel, by joining the opposite poles of the magnets, and placing them on the centre of the needle, so inclined that each formed an angle, as I afterwards ascertained, of about 30 degrees with the horizon. The magnets were then slid from the centre to the extremities of the needle, and their poles being again joined at a distance from the needle, the operation was repeated.

As I could in no way account for the diminution of directive force which I have remarked, except by supposing that I had inadvertently changed the inclination at which the magnets were held, I resolved to try whether a variation of this angle produced any considerable difference in the degree of magnetism communicated. For this purpose I re-magnetised the needles, by laying the magnets, with their opposite poles joined, flat upon the needle, the junction of the magnets being upon the centre. They were then separated and drawn to the extremities of the needle, the surface of the needle and that of the magnets being in contact the whole time. The poles were then joined and the operation repeated, using but little pressure.

Experiment 13.

Magnets moved flat upon the needle with little pressure.	Directive force.
Parallelogram	1265
Rhombus	1048
Pierced rhombus	1130

This manner of magnetising, therefore, appears much superior to that before employed.

The needles were again magnetised in the same manner as in the last experiment, except that the ends of the magnets were pressed pretty strongly against the needle.

Experiment 14.

Magnets moved as before, but with strong pressure.	Directive force.
Parallelogram	1263
Rhombus	1005
Pierced rhombus	1131

No advantage appears to have resulted from the increased pressure, but the arrow-shaped needle has suffered a diminution of power.

The magnets were now slid from the middle to the extremities of the needle at an inclination of only two or three degrees, and the following were the results:

Experi-

Experiment 15.

Magnets inclined in an angle of two or three degrees.	Directive force.
Parallelogram	1275
Rhombus	1051
Pierced rhombus	1150

This method appears to be preferable to any I have yet tried, and was therefore employed in the subsequent experiments of the present series. The rhombus of 63 grains, which in Experiment 10 was left with a directive force of 577, on being magnetised in this manner had its power increased to 600.

The ends of the needles were now hardened at a red-heat, the middle remaining soft, as before.

Experiment 16.

Needles hardened at the ends at a red-heat.	Directive force.
Parallelogram	1315
Rhombus	1020
Pierced rhombus	1185

Experiment 17.

Ends hardened at a bright red.	Directive force.
Parallelogram	1258
Rhombus	970
Pierced rhombus	1085

The ends hardened at a red-heat as near to that employed in Experiment 16, as possible.

Experiment 18.

Ends hardened at a red-heat.	Directive force.
Parallelogram	1350
Rhombus	1121
Pierced rhombus	1205

Here it should seem that an increase of power has been obtained by the ends of the needles having been first hardened at a higher temperature, and then at a lower.

The needles were now hardened throughout at a bright-red heat.

Experiment 19.

Hardened throughout at a bright red.	Directive force.
Parallelogram	1120
Rhombus	1205
Pierced rhombus	1080

The needles softened by laying them on a red-hot poker till they passed beyond the blue to a greyish white. This was carried to within an inch of their extremities, which remained hard.

Experi-

Experiment 20.

Softened from the middle to a greyish white; ends hard.	Directive force.
Parallelogram	1360
Rhombus	1140
Pierced rhombus	1210

The tempering was carried throughout the needles, the parallelogram was reserved for another purpose.

Experiment 21.

Softened throughout to a greyish white.	Directive force.
Rhombus	1075
Pierced rhombus	1145

Experiment 22.

Softened throughout to a greyish white, the ends hardened at a red-heat	Directive force.
Rhombus	1025
Pierced rhombus	1185

Experiment 23.

Hardened throughout, and then softened to a greyish white, as in Experiment 21.	Directive force.
Rhombus	1065
Pierced rhombus	1180

This last series of experiments presents a curious circumstance. From the experiments made by Coulomb, as well as from the general tenor of my own, the rhombus is found capable of receiving a greater directive energy than the parallelogram; yet here we perceive that the parallelogram, though formed of the very same plate of steel as the other needles, is not only under every circumstance superior to the rhombus, but also to the pierced rhombus. It is difficult to form any plausible conjecture as to the cause of this difference.

The weight of the rhombus in Experiment 10, made of clock-spring, was 63 grains; that made of saw-blade weighed 120 grains, or very nearly double. The directive energy of the former, after having suffered great deterioration, and when not tempered in the most favourable manner, compared with the greatest directive energy of the latter, was as 600 to 1210; but if we refer to Experiment 6, it may be seen that the greatest directive energy of the clock-spring rhombus was 814, which gives it an advantage of about one-third in directive energy over a needle of equal weight made of saw-blade.

From Experiment 20, it should seem that a needle is susceptible of the greatest directive power, other circumstances being similar, when it is hardened throughout at a red-heat, and then softened from the middle to within an inch of the extremities, till the blue colour which arises has again disappeared.

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I next proceeded to try, in a more regular manner, the effect of different methods of magnetising, and at the same time to ascertain whether the directive force was influenced by extent of surface, independent of mass. Two needles were made of the same kind of steel, in the form of right angled parallelograms, five inches long, the one 0·7 inch wide, and the other half this width. The widest was reduced in thickness until it was of the same weight as the other, viz. 142 grains. They were in the same state of softness as was necessary to work them. The magnets were placed together perpendicularly on the centre of the needle, their opposite poles being joined; their lower extremities were then separated and kept asunder, by placing a piece of wood, a quarter of an inch thick, between them, their upper extremities remaining in contact. The magnets were then slid along the needle, backwards and forwards, from end to end: this was repeated on both sides, till it was conceived the needle must be saturated.

Experiment 24.	Directive force.
Small parallelogram	655
Large parallelogram	674

The needles were again magnetised in the same manner as before, excepting that the magnets were separated at the top by a piece of wood of the same thickness as that at the bottom.

Experiment 25.	Directive force.
Small parallelogram	595
Large parallelogram	580

The magnets were placed perpendicularly together on the centre of the needle, and then their lower extremities separated by a piece of wood, to the distance of half the length of the needle, the upper extremities remaining in contact. They were then slid on the needle backwards and forwards, from end to end, as before.

Experiment 26.	Directive force.
Small parallelogram	760
Large parallelogram	780

The magnets joined, placed perpendicularly on the centre of the needle, as before, then moved in opposite directions from the centre to the extremities, keeping each magnet perpendicular to the needle; afterwards joined at a distance from the needle, placed again on its centre, and the operation thus continued.

Experiment 27.	Directive force.
Small parallelogram	993
Large parallelogram	1155

Remarking that the surface of the small parallelogram was unequal,

unequal, so as to be touched by the magnet in very few places, I filed it flat, and having reduced the large parallelogram to the same weight, they were magnetised by joining the magnets, placing them perpendicularly on the centre of the needle, separating their lower extremities, and carrying them to each end of the needle, the upper ends remaining in contact.

Experiment 28.	Directive force.
Small parallelogram	1025
Large parallelogram	1150

The needles were next magnetised, according to the method of Du Hamel, the magnets being inclined at an angle of about 45 degrees, and carried, as before, from the centre to the ends of the needle.

Experiment 29.	Directive force.
Small parallelogram	1070
Large parallelogram	1170

The magnets forming with the needle an angle of about 20 degrees.

Experiment 30.	Directive force.
Small parallelogram	1085
Large parallelogram	1195

Magnets forming an angle with the needle of about two or three degrees.

Experiment 31.	Directive force.
Small parallelogram	1160
Large parallelogram	1275

Magnets laid flat on the surface of the needle, and drawn from the centre to the ends.

Experiment 32.	Directive force.
Small parallelogram	1158
Large parallelogram	1261

Magnets forming with the needle an angle of two or three degrees, their other extremities being connected by a very soft iron wire.

Experiment 33.	Directive force.
Small parallelogram	1145
Large parallelogram	1261

The iron wire was now removed and the needle magnetised, as before, at an angle of about two or three degrees.

Experiment 34.	Directive force.
Small parallelogram	1160
Large parallelogram	1273

I now hardened both needles throughout at a bright red, and then

then softened them from the middle to within three-quarters of an inch of the ends till the blue had disappeared. This was done by laying the large parallelogram on a red-hot poker, but from the thickness of the small parallelogram this heat was found insufficient, and that of a lamp was employed. The needles were then magnetised as in the last experiment.

Experiment 35.	Directive force.
Small parallelogram	1815
Large parallelogram	1660

It occurred to me that the heat employed in tempering the large parallelogram might not have been sufficient, it was therefore exposed to the flame of the lamp, but in doing this, a small piece which weighed 10 grains was broken off from its end. It was, however, re-magnetised, and the directive force was now found to be increased to 1720.

From these last experiments, it appears that the greatest directive force was given to the needle when the magnets were inclined to it in an angle not exceeding two or three degrees, and that this force is little, if at all, influenced by extent of surface; as I conceive the small difference in favour of the greater surface may be attributed to some difference in the quality of the steel, or its temper, both of which appear to have very considerable influence on the directive force.

Two needles, the one five, and the other eight inches long, were cut out of the same plate of steel; they were of equal weight, the short one being of greater width than the other. Being magnetised to saturation, their directive forces were as follow:

Experiment 36.	Directive force.
Long parallelogram	2275
Short parallelogram	1193

They were now hardened at a red-heat, and tempered beyond the blue from the middle to within an inch of the extremities.

Experiment 37.	Directive force.
Long parallelogram	2277
Short parallelogram	1865

If the mean of these two experiments be taken, it will be found, as was observed by Coulomb, that the directive force of a needle of a greater length than five inches is probably as its length.

My next object was to repeat the very interesting experiments recently published by Mr. Barlow, proving the attraction of iron on a ship's compass to be dependent wholly on extent of sur-

 face.

face. For this purpose I had three cylinders made of soft iron, about two inches and a half in diameter, and nearly the same in height. One of the cylinders was of sheet-iron less than the 20th of an inch in thickness; the second of that kind called chest plate, 0·185 inch thick; and the third was of solid wrought iron. The first weighed 2760, the second 9376, and the solid cylinder 22929 grains. Previous to the experiments they were all made red-hot, to destroy any accidental magnetism.

The compass employed was of a very delicate construction, and the cylinder was so placed that its centre was in the direction of a tangent to the zero of the compass, and at the distance of 4·85 from the southern extremity of the needle. The position of the cylinder was varied six times, and the following were the deviations of the needle:

	Sheet iron cylinder.	Chest plate cylind r.	Solid cylinder.
	2° 15′	2° 50′	2° 55′
	2 15	3 4	3 15
	2 45	3 20	2 57
	2 5	3 45	2 50
	2 5	3 10	2 55
	2 10	3 30	2 30
Mean ..	2 16	3 16	2 54

Suspecting an error in the experiments with the solid cylinder from an accident which occurred, I repeated the whole with the utmost attention. The position of each cylinder was now varied eight times.

	Sheet iron cylinder.	Chest plate cylinder.	Solid cylinder.
	2° 3	2° 55	3° 15
	2 22	2 50	3 12
	2 32	3 20	3 15
	2 20	3 40	3 0
	1 50	3 40	3 15
	2 45	3 28	2 50
	2 45	3 10	2 45
	1 55	3 5	2 58
Mean ..	2 19	3 16	3 4

The surfaces of the cylinders determined by very careful measurement were, the sheet iron, 28·54; the chest plate, 30·77; and the solid cylinder, 28·94 inches.

Reducing the deviations to the same extent of surface, viz. that of the solid cylinder, they become respectively 141, 184, and 184 minutes.

These

These last results perfectly coincide with the deductions of Mr. Barlow, that the effect of iron on a ship's compass is as the surface, and is wholly independent of the mass; but that a certain degree of thickness of the iron (about two-tenths of an inch) is necessary to the complete development of this effect.

The following are the principal inferences which may be drawn from the experiments I have detailed:

That the best material for compass-needles is *clock-spring*; but care must be taken in forming the needle to expose it as seldom as possible to heat, otherwise its capability of receiving magnetism will be much diminished.

That the best form for a compass needle is the *pierced rhombus*, in the proportion of about five inches in length to two inches in width, this form being susceptible of the greatest directive force.

That the best mode of tempering a compass-needle is, first to harden it at red-heat, and then to soften it from the middle to about an inch from each extremity, by exposing it to a heat sufficient to cause the blue colour which arises again to disappear.

That in the same plate of steel of the size of a few square inches only, portions are found varying considerably in their capability of receiving magnetism, though not apparently differing in any other respect.

That polishing the needle has no effect on its magnetism.

That the best mode of communicating magnetism to a needle, appears to be by placing it in the magnetic meridian, joining the opposite poles of a pair of bar magnets (the magnets being in the same line) and laying the magnets so joined, flat upon the needle with their poles upon its centre; then having elevated the distant extremities of the magnets, so that they may form an angle of about two or three degrees with the needle, they are to be drawn from the centre of the needle to the extremities, carefully preserving the same inclination; and having joined the poles of the magnets at a distance from the needle, the operation is to be repeated ten or twelve times on each surface.

That in needles from five to eight inches in length, their weights being equal, the directive forces are nearly as the lengths.

That the directive force does not depend upon extent of surface, but, in needles of nearly the same length and form, is as the mass.

That the deviation of a compass-needle occasioned by the attraction of soft iron, depends, as Mr. Barlow has advanced, on extent of surface, and is wholly independent of the mass, except a certain thickness of the iron, amounting to about two-tenths of an inch, which is requisite for the complete development of its attractive energy.

LXXV. *On*

LXXV. *On the Apparatus for restoring the Action of the Lungs in apparent Death.* By John Murray, *F.L.S.M.W.S. &c.*

To Dr. Tilloch.

Sir, — I confess that I am not a little surprised at a communication by "Mr. John Moore junior," in your Number for March last. With sufficient self-complacency this correspondent considers the plan he proposes for restoring the action of the lungs as "more complete" than my invention. Mr. John Moore junior is pleased to adopt the form of the *syringe* which I had done long before; aye, and constructed and published too; and he even *condescends* to add, that "it might be well to enclose one of them (*i.e.* of the syringes) with a case to contain hot water similar to the description given by Mr. J. Murray."

I never believed myself infallible, or that my invention was incapable of improvement. I hope I am not so absurd or unreasonable; but I do fearlessly assert, that his *improvement*, as he insinuates it to be, is one which adds to the complexity of the mechanism without subserving its utility; nay, rather *injures* the cause it is meant to serve. Various plans presented themselves to my mind, before I completed my improved apparatus. A structure somewhat similar to the one now set forth and vaunted by your correspondent Mr. John Moore junior, was *immediately rejected*, from its complete uselessness. Because, until *natural respiration returns, the air impelled into the lungs and withdrawn, by the propulsion and retrogression of the piston, undergoes no change whatever,—ergo,* the idea for the necessity of a *renewal* of the air that is *not contaminated* is absurd. It was also abandoned on two other conditions;—the necessity of *valves*, liable to injury, unequal action, and occasional suspension;—and, that the instrument with these unnecessary incumbrances was more ponderous, complex, and heavy and difficult in operation.

There is no necessity whatever for *once* turning the stop-cock during the *entire* action of the machine, except to free the *bronchiæ* in the first instance, of the *stagnant air* reposing on their surface; or, in ordinary cases, to deposit a *drop of ether* in the lateral cell to be diffused into the included atmosphere, as an excitant to rouse the dormant resilience of the lungs; or, again, to receive a drop or two of hot water to impart *additional elasticity* to the atmospheric air in the cylinder, when the air is *too dry*.

I have said that the oxygenous part of the atmosphere cannot be contaminated until the flux and reflux of the blood are restored

stored by the return of the *natural* movements of the lungs and diaphragm ; and *then* the plugs being removed from the nostrils, the *mechanism of the apparatus is simply confined to* AID the incipient play of the lungs. Thus an inlet for *pure* air is instantly provided, when it is wanted,—and *before* this period there is no necessity whatever for new air.

I think your correspondent should have weighed these circumstances before he endeavoured to detract from the merits of the invention; and he should moreover have made himself acquainted with the structure of the instrument in its last and improved form.

As to the application of the instrument to the purposes of a "gas blowpipe," and the exhibition of "nitrous oxide;" I can have no ambition to claim an interest in such an association. The transition from the resuscitation of human beings, to a "gas blowpipe," &c. is so entirely ludicrous, that I am astonished such an erratic fancy should be indulged in.

I have little to add to my former observations. I continue to receive complimentary testimonials of the value of my invention; and it is cheering to add, that the apparatus, from its acknowledged utility, and superiority over the 'bellows" recommended by the Royal Humane Society, is about to be introduced into several infirmaries, &c.

It is singular, that all with whom I have conversed, unite in condemning the common apparatus as *inefficient*, to use the mildest term. I have not found *one* its advocate. It stands a rude and barbarous machine; a disgrace to the scientific genius of our country. The subject of restoring suspended animation has too long slept in inglorious repose; and it is one of the most important and commanding description.

In soliciting the aid of Science to a topic pregnant with such interesting results, I can be biassed by by no sinister motive, because I have no pecuniary interest to subserve. I desire, in return for my humble tributary aid in the cause of humanity, only a fibre of that feeling which vibrates in the breast of him, who is conscious of having done that which philanthropy approves,—what good men only know.

Surely the very numerous cases that have been unsuccessful, and where the appearances and circumstances were so flattering to the presages of hope, should have long ago convinced the Royal Humane Society of the inutility of the common means employed in resuscitation, and operated as a stimulus for the exertions of intellect, to improve the plan, and enlist Science into the cause.

When we see an obstinate adherence to the "bellows," notwithstanding its rudeness and imperfection, and even the probability

bability of its being injurious and destructive;—we may be inclined to adopt a severe, perhaps, but still a just reproof, "They are wedded to their idols, let them alone." The application of *tobacco smoke* in the mode recommended, is of a piece with the rest.

I certainly have reason to complain of the cold reception which my invention received from that quarter,—where it should have been *dispassionately* considered and cherished; and I boldly ask, Was it fair that three or four individuals in conclave should have pronounced such a hasty and inconsiderate opinion, before the merits of the invention were calmly weighed and discussed, when *twenty times* that number of gentlemen eminent in medical skill and scientific talent had on former occasions, without a single dissentient, pronounced it at once "ingenious" and "valuable?"—This sentiment of the Royal Humane Society has been certainly too inconsiderately promulgated.—These individuals *were assuredly bound* to state the *grounds* on which their opinion was formed and founded; viz. that the "bellows" were preferable for *general* purposes.

The *expense* and *complexity* of the machine were stated as difficulties to its "general" adoption. Now, as to the former, the bellows and its appendages complete cost 5*l.* 5*s.* My apparatus complete in all its parts may be supplied for little more than ONE THIRD of this sum. And as for complexity, I have said, it is simplicity itself. Where is the difficulty of operating the piston? A child need not be taught it; it is not at all comparable with the bellows, even in this respect, and in its advantages *infinitely superior*, and armed at all points for *every case* of asphyxia. If the explanatory remarks accompanying the presentation of the instrument to the Royal Humane Society were imperfect or insufficient, I was ready to re-explain and re-extend.—I have no doubt that the valuable Society in question will see the *necessity of reconsidering* their opinion, which otherwise might tend most materially to injure their own glorious cause, and prevent useful communications connected with the important question of life.

Be it remembered, mine are not *theoretic* speculations, but that the instrument was the offspring of rigorous induction. I have never succeeded in a *solitary instance* in restoring the action of the lungs in inferior animals, by propelling the air through a *tube encased with ice*. But with air *heated to the animal temperature* my success has been such as to enable me, on just and proper grounds, most heartily to recommend the plan now proposed. In *four* out of five cases it has been successful.

My experiments inform me, that there is little probability of the air being forced down the Œsophagus instead of raising the Epiglottis,

Epiglottis, and passing through that channel. At any rate, a ribbon of tape tied lightly round the neck will prevent it.

The stop-cock when required in action is managed as quick as thought.

I am sorry that my avocations prevent an extension of these observations. I shall for some time take leave of the subject of respiration and suspended animation, and content myself with recommending in my prælections the invention I have proposed, stating always the *grounds* of that recommendation.

I have the honour to be, sir,

Your obliged and most obedient,

May 10, 1822. J. MURRAY.

LXXVI. *On Spade Husbandry.* *By Mr.* WM. FALLA.

[The following letter from Mr. Falla, detailing the experiments of four successive years in the cultivation of wheat by the spade, was addressed to Mr. Owen, of Lanark, and is published by him in an appendix to "A Report to the county of Lanark of a plan for relieving public distress, and removing discontent, by giving permanent productive employment to the poor and working classes." We republish it, because the state of agriculture is such at this moment, that it is of the utmost importance to give every possible publicity to all plans calculated to lighten the burdens, or in any respect alleviate the sufferings of the large portion of the population engaged in agricultural pursuits; and none can add so much to the stock of knowledge on this subject, as men like Mr. Falla, who take truth and experience for their guides, and who never cease to prefer facts to theories—and a little practical good, to a great deal of hypothetical advantage.]

Gateshead, Newcastle-upon-Tyne, Nov. 13, 1820.

DEAR SIR,—BEING persuaded that it is a subject of the very first importance, I readily obey your request to furnish you with the particulars and results of my experiments in the cultivation of land, for the production of wheat, by the spade. It may not be without its use, previously to detail to you the circumstances that brought my attention to this subject. I therefore take the liberty to state, that my principal occupation, for between thirty and forty years past, has been the cultivation of land, chiefly for the raising of trees and seeds for sale; and finding, as I was extending my concerns in that way, about sixteen or eighteen years ago, a difficulty in procuring a sufficient number of men to work the land with the spade, I substituted the plough in working those parts where a considerable quantity of vacant ground happened to lie together, and fancied, that, besides getting through the work with more facility and convenience, which I certainly did, I was doing the work in a manner equal to work done with the spade. The effect of the first use of the plough was not of so much bad consequence as when repeated; the beating of the

subsoil by the horses'-feet, together with the action of the iron bottom of the plough, not having at first the miserable effect of making the bottom of the worked ground hard and firm, like a turnpike road; the continued successive use of the plough, however, soon showed the bad effect, in the diminished health and vigour of the trees, &c. Fortunately this observation was made when men for spade work were easier to be obtained, than at the period when the use of the plough was adopted, and in part then, but entirely since, I have laid it aside in all my nursery operations.

In the use of the spade I produce a depth of well-worked earth of nine to ten inches, which is more than twice that of the plough, as used in the counties of Durham and Northumberland; and instead of the hardened level bottom, not easily, if at all, penetrable, in our strong clayey subsoils, by either superfluous moisture, or the roots of plants, I obtain a loose broken bottom, conceived to be a particularly favourable circumstance in such soils.

Soon after, or rather during the time that my practice was changing from the use of the plough to that of the spade, I received a letter from a gentleman of great respectability, and accurate observation, in Yorkshire, expressing himself strongly impressed with an opinion, that if garden culture with the spade were introduced into farming, very great addition might be made to the produce of the said land as worked by the plough; and that the full energies of the land will never be called forth till the spade is made to supersede the plough; asking for my opinion and any observations I might have made on the subject, detailing, at the same time, the particulars of an experiment in wheat with spade culture, which had been made a good many years before, at Nottingham, the produce of which was beyond all example. This information, so strongly corroborating my own observations, confirmed me in my practice of the use of the spade for nursery purposes, and stimulated me to the extension of it, and to the making of experiments of the same kind. The Nottingham experiment having been made with plants of wheat raised upon garden beds, and from thence transplanted into lines, I began with an adoption of the same mode; I sowed the wheat in beds in the month of August, and transplanted the same in September and October,—the distance of the lines from each other was, in one experiment, nine, and in another twelve inches—placing, in both cases, twelve plants per yard in the lines. These experiments I made two successive years, and the least produce was fifty-two bushels, and the greatest sixty bushels, Winchester, per acre. The quantity of ground under these experiments was half an acre each year, which I think may be considered

sidered a pretty fair quantity for an experiment; perhaps a much smaller one would not be so.

The digging, as at my common nursery price, costs fourpence per rood, of forty-nine square yards (the rood of this country) or thirty-three shillings per acre; the transplanting, fourpence half-penny per thousand; but there is a great saving of seed, from one to two pecks of wheat producing as many plants as are sufficient to plant an acre, whereas the usual quantity for plough cultivation, sown broadcast, is eight pecks, or two bushels per acre. The following, on these *data*, is a calculation of the expense of cultivating one acre in this way, supposing the lines nine inches asunder:

	£	s.	d.
Digging	£1	13	0
Transplanting 232,323 plants at 4½*d.* per 1000	4	7	1½
Two pecks of seed wheat	0	4	6
Total ..	£6	4	7½

During the time of making these experiments, it occurred to me, that probably the increased quantity of wheat, produced in this way, arose more from the deep working of the land by the spade, than from the circumstance of transplantation; and I added to the transplanting experiments, for the two past seasons, others, in which the wheat was sown both in drills and broadcast, the land in all the cases worked in the same manner by the spade, and the following are the results:

Crop 1819.

				bushels p. acre.
No. 1 transpl. from the seed-bed into	6 in.	lines,	produced	62½
—— 2 do.	9	do.	do.	56¾
—— 3 do.	12	do.	do.	61
—— 4 sown in drills	9	do.	do.	65⅘
—— 5 sown broadcast			do.	58⅗

Crop 1820.

No. 1 transpl. from the seed-bed into	6 in.	lines,	produced	68¼
—— 2 do.	9	do.	do.	68¼
—— 3 do.	12	do.	do.	60¾
—— 4 sown in drills	9	do.	do.	73½
—— 5 sown broadcast			do.	76¼

I must here state, that a portion of No. 4, in the last detailed set of experiments, was laid down by wet, when in flower, and proved very abortive, otherwise I have little doubt that No. 4 (as in the former year) would have exceeded No. 5 in quantity; and a considerable part of the wheat of Nos. 1, 2, and 3, was shaken out by the wind, and destroyed by birds, to the amount probably of five or six bushels per acre.

With relation to denominations of Winchester measure, com-

 pared

pared with those of Scotland, I have to observe, that the Winchester bushel contains thirty-two quarts, and the quarter eight bushels, also that a boll Linlithgow, or Edinburgh measure, contains, within quite a small fraction, four bushels Winchester.

I have already stated the expense of cultivating by spade work, and transplanting from a seed-bed, in lines nine inches asunder, one acre of wheat; I will now state the expense of one acre in drill, and also broadcast:

Digging	£1	13	0
Seed wheat, two bushels per acre	0	18	0
	£2	11	0
If sown broadcast, and the seed is harrowed in by a horse, say 2*s*. per acre; if raked in with a garden rake, it will cost	0	4	0
	£2	15	0

If sown in drills, and the drills made with a garden hoe, it will cost 4*s*. per acre more, but a larger saving than that expense will be made in the quantity of seed, compared with the broadcast method.

I now take the liberty to state, what I conceive is the comparative expense of cultivating an acre of land by the plough, and in the first place I have no difficulty in asserting, that one digging, as I have it done (leaving the extra depth out of the question at present) is equal to three ploughings and harrowings; I believe I may also state, that the ploughing each time of an acre is calculated to cost 8*s*. and the harrowing 2*s*.—If this is allowed, an acre in this way costs

Three ploughings and harrowings, at 10*s*.	£1	10	0
Seed wheat, two bushels per acre ..	0	18	0
Harrowing the seed in	0	2	0
	£2	10	0

Thus it appears that the cultivation of an acre of wheat by the spade, costs only 5*s*. more than by the plough. In respect to the comparison of expense between wheat *transplanted* and *sown* on land worked by the spade; from the two last years' experiments (the expense of transplanting being of course taken into the question) there can be no doubt that sowing is the better system, and that the advantage over the plough, is from the deep and otherwise superior working of the land by the spade.

The comparative advantage of *produce* is now to be stated; the average produce of wheat of the whole island, taking an average of seven years, is said to be twenty bushels per acre. The

The average of my neighbourhood, I believe, is about twenty-four bushels, but instead of making that a criterion by which to make the comparison, I have to state, that in the autumn of 1819 a good deal of pains was taken to ascertain the quantity of wheat upon a field immediately adjoining my land, and which was what is considered a remarkably fine crop, by which it appeared to be thirty-eight bushels per acre; this was on land, although adjoining, yet of a naturally better quality than mine, and quite as highly manured, worked, in the usual manner of this country, with a two-horse plough, and sown broadcast. By inspection it will be seen, that the average quantity of my drilled and broadcast experiments in 1819 and 1820, is 68½ bushels per acre: the value of seed wheat has been assumed to be 9*s.* per bushel, I will however for a whole crop take it lower, say 8*s.* per bushel; the comparison in respect to value will then stand thus per acre:

By the spade, 68½ bushels per acre at 8*s.*	£27	8	0
By the plough, 38 bushels per acre at 8*s.*	15	4	0
The difference is	£12	4	0

being an advantage gained by the extra expense of 5*s.*

It is of much importance, on this very interesting subject, that every circumstance connected with the experiments should be known; I therefore state, that the quality of my land on which they were made, although naturally poor, is of that middle texture that will grow the two extremes of turnips and beans; that, at the distance of 10 or 12 miles from Newcastle, it would be let for, at most, 30*s.* per acre; that when I got possession of it, there were not above 4 to 6 inches of earth, upon a subsoil of clay; that every year it has been worked, I have brought up to the surface a small quantity, say one inch of the said subsoil, and that I have now a depth of earth of one foot, the whole equal, or more than equal, to the quality of the 4 to 6 inches upon it, when I first had it;—further, that my experiments for crop 1819 were made after a crop of turnip *seed*, the land previously manured for the turnips, before the seed was sown, after the rate of 20 tons of stable dung per acre, no additional dung used for the turnips, when transplanted, nor for the wheat crop, the plants and seeds respectively, for the different experiments of which wheat crop, were planted and sown at the same time in September. The land upon which the experiments for crop 1820 were made, had previously upon it a three years' crop of transplanted larches, which of course not a little exhausted it; the larches were followed by turnips for seed, a two years' crop, as in the former case, and, as will be allowed, a very exhausting one; this land had an allowance of 20 tons of stable manure per acre, applied

applied when the turnip seed was sown, and no more added when they were transplanted: but, considering the state of the land, from the effect of the larches and the turnip seed, it was thought that justice would not be done to the wheat without an application of a smaller portion of the same sort of manure, and I gave it 10 tons per acre.

I have not yet made any experiments, by spade culture, on oats and barley, but I am intending to make one or more upon each of those grains, and perhaps on beans, the ensuing spring: I am at present digging part of one of my fields for that purpose, the results of which shall be detailed to you.

Being desirous of ascertaining how far, and at what expense, it may be practicable to work land by the spade, by women, boys, girls, and feeble old men, in order, among other reasons, to the employment of paupers of that description, in which, alas! this country, south of the Tweed, superabounds; I have this autumn made an experiment on a piece of land containing 1728 square yards, by digging or rather *trenching* by two short spits with girls, and I have the pleasure of saying that the work is better done by two such short spits, each about five to six inches deep, the one following the other, than digging is done by men at one full spit or spade full, about nine to ten inches deep. The common wages I pay to these girls is 10*d.* per day, and they did the work in nineteen days, for one girl, which cost 15*s.* 10*d.*: —an acre at the same rate, containing 4840 square yards, would cost 2*l.* 4*s.* 4*d.*, this is 11*s.* 4*d.* per acre more than by men at one spit, but I am satisfied that the superiority of the girls' work is well worth the difference: I may add, that this being the girls' first attempt with spades, I am persuaded that by further practice they would in a short time do it for the men's price, 33*s.* The girls work with quite light spades made for the purpose, the best size for which I think to be 9¼ inches long, 8 inches broad, and weighing, with the light handle, about 4½ lbs. avoirdupois.

A few months ago I took the liberty of stating to you, that as a *parochial* concern, for the employment of the poor, at present dependent on their respective parishes for relief, your system might be adopted with very great effect; and one principal object, as I have already said, in making the last detailed experiment, was to ascertain how far it is practicable to employ, in the cultivation of the soil, persons who are so dependent on parish relief, of the descriptions of women, boys, girls, and feeble old men, at present doing little more than sitting over the poor-house fire; the greatest part of whom may, as it is now ascertained, be employed to great effect in the heaviest manual labour, in the cultivation of the soil; and of course in the easier

operations

operations of hoeing, weeding, &c. I think I may venture to add, that there need be little doubt entertained that there are few even of such, at present, miserable objects, who would not be able in that way to earn a maintenance, and, were such a measure generally adopted, the poor's rates in England, at present said to amount to eight millions, reduced to perhaps one fourth of that sum. A better arrangement might probably be thought of than what has occurred to me, which is, that the parish, according to the extent of its wants, shall purchase, say from twenty to fifty or more acres of land, build upon it cottages to the necessary extent, employ a proper person to lay out the ground in the best manner for the purpose, see the poor set to work, and that they do the same in a proper manner through all its operations; also that each does a day's work, according to individual ability; and that such as are not able to dig, rake, &c. be employed in other more easy operations, as the weather and their ability may permit.

Before I conclude, there is one more strong argument in favour of spade husbandry which must be noticed. As far as that mode may be adopted, there will of course be a saving of land for the production of food for man, which is now appropriated to the keeping of horses; and I believe that few persons are aware, that the quantity of land necessary for the keeping of a horse is, as may be very easily made to appear, $4\frac{1}{2}$ acres; I am meaning a quality of land similar to mine, as already described; which quantity, it may be very clearly made to appear, will afford subsistence for *nine persons*, on the supposition of a common proportion of men, women, and children, and this under the husbandry of the plough: but on the supposition of spade culture, that quantity of land will produce sufficient subsistence *for more than twelve persons*.

Should it be objected that a serious inconvenience may arise from the want of the present supply of manure from horses, the difficulty will be easily obviated by keeping more horned cattle, and by means of an almost religious attention (as in China) to the preservation of perhaps the best and most powerful of all manures, human urine, which at present is, in this island, almost entirely lost.

I am, with sentiments of the greatest respect,
dear sir, very sincerely yours,

WILLIAM FALLA.

To Robert Owen, Esq.

LXXVII. *No-*

LXXVII. *Notices respecting New Books.*

Recently published.

The Fossils of the South Downs; or Illustrations of the Geology of Sussex. By Gideon Mantell, F.L.S. G.S., Fellow of the Royal College of Surgeons. Royal 4to, with 42 Plates. 3*l*. 3*s*. in Boards.

A New and Classical Arrangement of the Bivalve Shells of the British Islands. By W. Turton, M.D. 4to, with 20 Plates, drawn and coloured from Original Specimens in the Author's Cabinet. Price 4*l*.

Remarks on the present Defective State of the Nautical Almanack. By Francis Baily, F.R.S. and L.S. 8vo. 2*s*.

Practical Observations on the Nautical Almanack and Astronomical Ephemeris, with Arguments proving that it was not originally designed for the sole Improvement of Nautical Astronomy. By James South, F.R.S. 8vo. 4*s*.

First Elements of the Theory of Series and Differences, being an Attempt to combine into one harmonious Whole, resting upon the simple Basis of Addition and Subtraction, the several Theorems taught in this important Branch of Mathematical Science by Pascal, Newton, Taylor, De Moivre, Lagrange, and others. 4to. 18*s*.

Observations on a General Iron Rail-way: with a Geographical Map of the Plan, showing its great superiority, by the general Introduction of Mechanic Power, over all the present Methods of Conveyance by Turnpike Roads and Canals, and claiming the particular attention of Merchants, Manufacturers, Farmers, and indeed every Class of Society. 8vo. 6*s*. 6*d*.

A Treatise on the Diseases of Arteries and Veins; containing the Pathology and Treatment of Aneurisms and Wounded Arteries. By Joseph Hodson, Member of the Royal College of Surgeons in London. 8vo. 15*s*.

An Experimental Inquiry into the Laws of the Vital Functions; with some Observations on the Nature and Treatment of Internal Diseases. By A. P. Wilson Philip, M.D. F.R.S. E. 8vo. 10*s*. 6*d*.

A Celestial Atlas, comprising a Systematic Display of the Heavens, in a series of Thirty Maps; illustrated by scientific Descriptions of their Contents, and accompanied by Catalogues of the Stars, and Astronomical Exercises. By Alexander Jamieson. Royal 4to. 1*l*. 5*s*.

A Journey from Merut, in India, to London, through Arabia, Persia, &c. in 1819, 1820. By Lieutenant T. Lumsden, of the Bengal Horse Artillery. 8vo. 10*s*. 6*d*.

Illus-

Illustrations of the Universal Efficacy of Compression and Percussion in the Cure of Rheumatism, Sprains, &c. By Wm. Balfour, M.D. 8vo. 2*s.*

A New System of National and Practical Agriculture. By R. Donald. 2*s.* 6*d.*

Tracts on Vaults and Bridges. 8vo. 1*l.*

A Description of the Topography of the Plain of Troy. By Charles Maclaren. 9*s.*

In the Press.

An Introduction to the Study of Fossils, in a Compilation of such Information as may assist the Student in obtaining the necessary Knowledge respecting these Substances, and their Connexion with the Formation of the Earth. By James Parkinson, Author of the Organic Remains of a former World.

The First Part of Dr. Hooker's Exotic Flora will be published on the 1st of June.

Legendre's Elements of Geometry, and of Plane and Spherical Trigonometry. Edited by Dr. Brewster, of Edinburgh.

LXXVIII. *Proceedings of Learned Societies.*

March 28. On the Anatomy of Whales. By W. Scoresby, Esq.

April 18.—On the Changes that have taken Place in the Declination of some of the principal Fixed Stars. By John Pond, Esq.

Extract of a Letter from Capt. Sabine to the President.

Some Observations on the buffy Coat of the Blood.

April 25.—On the Nerves. By Charles Bell, Esq.

ASTRONOMICAL SOCIETY OF LONDON.

May 10. A communication was read from Capt. Everest, on the subject of the geodetical operations carried on by M. Lacaille, at the Cape of Good Hope, in the middle of the last century. These inquiries were suggested by Col. Lambton; and Capt. Everest has examined the subject with that accuracy and research, which give promise of much valuable information from the same quarter. Capt. Everest has identified the principal stations which Lacaille occupied in the course of his operations; and has clearly ascertained the house in Cape Town where his observatory was fixed. This latter point, as well as the one at the northern extremity, were, from their proximity to very high mountains, very unfit for such nice operations: and Capt. Everest conceives that those immense masses of rock must have had a

sensible effect on the plumb line of the instrument. Added to which, the whole plain, in which the measurement was effected, was also unfit for such a purpose, except with instruments which have been employed at much later periods. From these and other causes Capt. Everest thinks that this celebrated measurement ought not be considered (without a more recent corroboration) as sufficient evidence of an inequality in the two hemispheres of our globe. We have no doubt this subject will engage the attention of our astronomer at the new observatory in that country.

Some new instruments, of a peculiar mechanism, were exhibited to the members present.

The next meeting of the Society (which will be the last of the present sessions) will take place on Friday, June 14, at 8 o'clock in the evening.

[In our last account of the proceedings of this Society, we remarked that Sir Thomas Brisbane had observed, at sea, an occultation of *Mercury* by the Moon. We apprehend this must have been a mistake in the paper transmitted to the Society, as *Mercury* was not (at the period alluded to) in such a situation as to admit of his being occulted. It was probably *Regulus* that was occulted: and that the transcriber had inadvertently written the name of the planet, instead of the star.]

CEYLON LITERARY SOCIETY.

At the monthly meeting of this Society, which was held on the 17th of September 1821, His Excellency the Lieutenant Governor in the chair, the following paper by Lieut. Col. Wright was read:

Observations on the Barometer as applicable to the Island of Ceylon.

The scale of variation in the Barometer being of a very limited nature between the tropics, compared with that of latitudes at a greater distance from the equator, makes that valuable instrument, in general, be considered, especially by superficial observers, as of little service in the former case; yet there is no doubt but by an attentive and careful observation it may be made subservient to many useful purposes, and become in the hands of the agriculturist and navigator an equally valuable instrument even in low latitudes. It is only necessary to know its scale and its language. A sudden fall of two or three tenths of an inch of the mercury in the tube is probably the prognostic of as great a change in the atmosphere as the fall of as many inches in some other parts of the world; and as the observation is as readily made in one case as the other, it becomes of importance to be noted.

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The following remarks and observations, made during a period of several years in Ceylon, are offered, not with a view of establishing any fixed principle with regard to the above instrument, and of the laws by which its movements are regulated, but more to serve as general hints in any future observations that may be made, and to afford the opportunity of forming comparisons therein with any observations made in other parts of India and between the tropics.

At Colombo, which lies in latitude 6° 56′ North, and close on the sea shore, the Barometer appears decidedly to undergo four periodical changes or revolutions in the course of twenty-four hours, amounting in general to about one-tenth of an inch, being highest about nine o'clock in the morning, sinking towards three in the afternoon, rising again towards nine at night, and sinking again towards three in the morning.—There does not appear to be any sensible difference between the position of the mercury in the tube in the morning and at night—the point at which it stands in the morning being generally the same as at night.

Heavy rains do not affect the Barometer in an equal degree proportionally with that in high latitudes, nor do hard squalls of a sudden nature or short duration affect it any more than in other parts of the world; but a smart gale of wind of any strength and continuance will sink the mercury to the extent of about three tenths of an inch; and though that change may not take place so great a period of time previous to the gale commencing as in other latitudes, yet still, by a careful and attentive observation it will give a sufficient warning of the approach of a gale, so as to prove of very great utility to ships at anchor in the roads of Colombo, or off the coast. In the month of November 1819, previous to the commencement of a smart gale of wind from the north-west, the mercury, which had been at 29.9 inches, fell to 29.7, with the Thermometer at 76° of Fahrenheit, and remained low during the continuance of the gale, and gradually continued rising previous to the gale abating, and in several similar instances it has never been known to fail.

The variations in the rise and fall of the mercury do not appear to be affected in any remarkable manner, or influenced by heat or cold, or to undergo any changes with the Thermometer in similar cases, but it appears to stand highest in steady, fixed, settled weather. The different monsoons do not appear to affect it, though at the changes thereof a variation takes place in its rise and fall.

The average height of the mercury throughout the year may be considered as about 29.9 inches; the highest range 30.1 nearly, and the lowest about 29.7, making the greatest range somewhat near half an inch; and this observation may be considered

dered as applying to Barometers on board the ships in the roads and off the coast, as the difference probably is very trifling between those and Barometers on shore and near the sea coast on a low elevation.

No sensible difference has hitherto been observed in the Barometer on the western and eastern sides of the island; for, at the time of a gale of wind on the western side, during the south-west monsoon, the same changes occur in the rise and fall of the mercury on the eastern side, and *vice versâ*.

In the city of Kandy situated at the distance of about eighty miles inland, and at a computed elevation of about 2500 feet above the level of the sea, during the month of October, the maximum of the Barometer, while the Thermometer was at 76° of Fahrenheit, was 28.452 inches, and the minimum while the Thermometer was 70° was 28.272. Sufficient observations have not as yet been made to determine with accuracy the general average height, but it may be considered as about 28.3 inches; and similar to what occurs at Colombo, it is always higher in the morning about nine o'clock, and at night, than at the hour of three. In fact, this periodical rise and fall of the mercury appears of so fixed and established a nature, that, there is no doubt, an attentive observer of the Barometer may thereby mark the above hours and intervals of time with very tolerable accuracy, where the state of the atmosphere and the weather has not during the time of observation undergone any very material change.

The following additional remarks and observations on the Barometer, though not applicable to this island, may notwithstanding be deemed not unworthy of a place in the Transactions of the Ceylon Literary Society.

At the Mauritius or Isle of France, in the month of January 1819, the mercury in the Barometer falling to 29.10 inches, was followed by a very violent hurricane, and as the gale abated, the mercury again gradually rose and continued rising till it reached 29.80 inches, the Thermometer of Fahrenheit during the time of the gale varying from 75 to 81 degrees.

At the town of Port Louis in the month of February, being the middle of summer, while the average height of Fahrenheit's Thermometer was 86°, that of the Barometer was 27.7¼ in French inches and lines; the English foot being to the French as 12 is to 12.816.

At Madras in the month of October 1818, the mercury in the Barometer fell to 28.78 inches, which was considered as unprecedented at that place, and was followed by a very violent gale of wind, which gradually abated as the mercury continued to rise until it reached the height of 29.8 inches, which it had been

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at the previous part of the day. The Thermometer during the time of the gale was in general about 74 degrees: and at the same place in the month of May 1820, the mercury fell eight-tenths of an inch below the height which usually indicated a gale of wind, and was accompanied by a very heavy gale and an unusual fall of rain.

Off the Cape of Good Hope, the mercury in the Barometer falling down to 29.60 inches is almost invariably the prognostic of a storm—the usual average height is that of about 30 inches, and to which height it again gradually rises as the gale abates, and continues at that elevation while the weather is serene and fair. A good Marine Barometer is there of absolute and essential service, as these gales often come on suddenly without any remarkable change in the appearance of the heavens or atmosphere, but are invariably foretold by the Barometer. It is however to be observed, that the steady strong breezes almost approaching to a gale, and which blow there from the south-east in the summer season, have a tendency to raise instead of sinking the mercury. In that latitude it is not ascertained if the periodical changes already alluded to take place the same as at Ceylon, though probably not, as that very extraordinary and unaccountable circumstance appears to be confined to the tropics and equatorial region. The mercury there has been observed during the month of May to rise to the height of 30.4 inches nearly, but the average height may be considered, as above stated, 30 inches in general.

LXXIX. *Intelligence and Miscellaneous Articles.*

ON THE FUNCTIONS OF PROGRESSIVE MOTION IN VERTEBRATED ANIMALS.

In one of the lectures on Comparative Physiology delivered this season at the Royal Institution by Dr. Roget, he gave an account of the functions of progressive motion in vertebrated animals, a division that includes the classes of fish, reptiles, birds, and quadrupeds; all of which, he observed, however different their external form, or the nature of the element they inhabit, exhibit nevertheless a remarkable analogy in their internal conformation. He took a general view of their mechanical structure, more especially with reference to the osseous frame work, or skeleton, which characterizes this division of the animal kingdom. That part of the skeleton which exists in all these animals, and appears to be essential to it, is the spine, or that connected series of bones called vertebræ, extending from the head along

along the whole length of the back. The peculiar mechanism by which the spine is rendered capable of answering a variety of important purposes in the animal system, was fully explained; and the several modifications of structure pointed out, which it receives in different tribes, in order to adapt it to the circumstances in which they are placed, and to the various intentions of their formation. The system of organs by which the locomotion of the body is effected, was next considered, in the relation which they bear to the element on which they are exerted.

As aquatic animals present the simplest mechanical conditions with reference to locomotion, Dr. Roget began with the examination of this function in fishes. He observed that the buoyant force of the fluid which surrounded them, by counteracting nearly the whole of the force of gravity, superseded the necessity of limbs for the support of the body, which land animals require; and that the progress of a fish in the water is effected principally by the muscular action of the tail, which, giving powerful lateral strokes, impels the animal forward on the same principle that a boat is moved in sculling. The modifying and regulating powers of the fins were next explained, and elucidated by diagrams and drawings. The hydrostatic principles on which fishes are assisted in their ascent or descent in the water, by the dilatation or compression of the air-bladder, were stated, and illustrated by some experiments, in which similar effects were produced in glass vessels immersed in water, but containing sufficient air to enable them to rise to the surface, or sink to the bottom, according as the included air was made to expand or contract. The air bladder in fishes may be regarded as a refined apparatus in the body of these animals, expressly accommodated to the laws of hydrostatic pressure; and as furnishing one out of the many instances that exist, where philosophical principles have been applied, with manifest art and intention, for effecting a particular purpose in the œconomy. Those fishes which have no air-bladder, as flat fish, have this want compensated by the great size and power of motion in the pectoral fins, which enable them to strike the water from above downwards with considerable force. In the whale, and other animals of the cetaceous tribe, the body is rendered specifically lighter by the large quantity of oil which it contains, and which is especially accumulated about the head, as this part of the body is continually required to be raised above the surface, for the purpose of respiration. The various modes of progressive motion employed by other aquatic tribes, both of reptiles and of mammalia, were also noticed.

Dr. Roget next proceeded to consider the mechanism of land animals, beginning with serpents, and reptiles having short and imperfect feet. He showed the means by which the former are enabled

enabled to advance with various degrees of rapidity, and the advantages they derive from the position of their scales, and their connexion with the ribs. The tortoise, the frog, the lizard, and various other animals of the same class, were noticed as forming links in the chain of gradation leading to the more perfect conformation, with reference to rapid progressive motion, which obtains in warm-blooded quadrupeds. In these, the body being raised higher on the limbs enjoys a greater range of motion, and requires a less frequent repetition of steps in traversing an equal space. The different proportions in which the weight of the body is sustained by the fore and hind extremities, the number of levers of which they are composed, their relative obliquity, the mode in which the muscular force is disposed, and the combinations of action which result, were severally explained. A particular account was given of the paces of quadrupeds, such as walking, the trot, the gallop, the amble; the bounding of deer, the springing of beasts of prey, the undulating pace of the camelopard, and the peculiarities in the progression of animals formed for leaping, as the hare, the jerboa, and the kangaroo. A gradation was pointed out in the structure of the hind foot, which, in monkeys, makes the nearest approach to the human structure. Dr. Roget observed, that the great features of distinction between the mechanism of the human frame and that of quadrupeds, are derived from the former being adapted to the maintenance of the erect posture. In man, the attitude of standing is a position of less security than it is in quadrupeds, and is maintained by a succession of actions by which the centre of gravity is perpetually shifted from side to side; its tendency to fall in any one direction being immediately counteracted by small and insensible movements in the contrary direction. On this principle he also explained the security of the rope-dancer. The human arm, being exempted from the office of supporting any part of the weight of the trunk, may be employed exclusively as an organ of apprehension; and the circumstances in the structure of the several joints of the limb, and more especially of the hand, which render it so admirable a mechanical instrument, were fully pointed out. The passage of the tendons by which the last joints of the fingers are bent, through a perforation in those which are employed to bend the middle joints, was particularly selected as an example of artificial contrivance.

The progressive motion of birds was the next object of inquiry. In order that an animal may possess the faculty of flying, two principal conditions, it was observed, are required: first, great strength of muscle to produce sufficient velocity of motion in the wing; and, secondly, great extent of surface in that part of the wing destined to act upon the air. None of the mammalia, except

cept the bat, has sufficient muscular power in its limbs, however assisted by an expansion of surface, to strike the air with the velocity requisite for flight. Some quadrupeds, reptiles, and even fish, possess the power of advancing through the air, but always in a very limited degree. It is in the bird alone that we find the most perfect adaptation of structure to the purposes of flight. The frame of their skeleton, the position and figure of the wings, the situation of the muscles, and the mechanism of their action, were severally pointed out as having an express relation to the element in which nature intended them to move; and the various modifications which these circumstances present in the different orders of birds were particularly specified. The minute structure of the feathers, when investigated by the help of the microscope, appears highly curious, and exhibits a singular refinement of art in the means by which their fibres are mechanically locked into each other, so as to preserve a continuity of surface. The singular mechanism by which birds sustain themselves by means of one foot on their perch, when they roost, was also detailed. Several skeletons of birds and quadrupeds were exhibited in illustration of the leading points considered in these lectures, which close the subject of the progressive motion of animals.

RUSSIAN DISCOVERIES.—COCHRANE THE TRAVELLER.—IMPERIAL UKASE.

Since the general peace of Europe, and more particularly within the last three years, the Russian Government has been anxiously and eagerly employed in prosecuting discoveries in every part of the globe. In the Southern Ocean, her ships have penetrated the fields of ice as far as the seventieth parallel of latitude, and discovered, it is said, islands which had escaped the searching eye of Cook: they boast of having rounded the Sandwich land of that celebrated navigator; and of having ascertained that the Southern Shetland, which was supposed to be a continent connected with it, consists only of numerous groups of small islands. They have sent land expeditions into the unknown regions of Tartary, behind Thibet, and into the interior of the north-western side of North America. Men of science have been commissioned to explore the northern boundaries of Siberia, and to determine points, on that extensive coast, hitherto of doubtful position. In February 1821, Baron Wrangel, an officer of great merit, and of considerable science, left his head-quarters on the Nishney Kolyma, to settle, by astronomical observations, the position of Shalatzkoi-Noss, or the north-east cape of Asia, which he found to lie in lat. 70° 05′ N. considerably lower than it is usually placed on the maps. Having arranged this point, he undertook the hazardous enterprise of crossing

crossing the ice of the polar sea on sledges drawn by dogs, in search of the land said to have been discovered, in 1762, to the northward of the Kolyma. He travelled directly north, eighty miles, without perceiving any thing but a field of interminable ice, the surface of which had now become so broken and uneven, as to prevent a further prosecution of his journey. He had gone far enough, however, to ascertain that no such land could ever have been discovered. The idle speculation, therefore, of the junction of Asia with North America, which we always rejected as chimerical, may now be considered as finally set at rest. Indeed, the simple narrative of the voyage performed by Deshnew in the year 1648, from the mouth of the Kolyma to the gulf of Anadyr, never, for a moment, left a doubt on our minds of its authenticity.

Information was recently received that the enterprising pedestrian Captain Cochrane had reached the Altai mountains, on the frontier of China. Further accounts from this extraordinary traveller have since reached us; they are dated from the mouth of the Kolyma, and from Okotsk, the former in March, the latter in June 1821. He had proceeded to the neighbourhood of the North-east cape of Asia, which he places half a degree more to the northward than Baron Wrangel; but either he had no instrument sufficiently accurate to ascertain its latitude with precision, or, as we have some reason to believe, he states it only from computation; for it does not clearly appear from his letter to us that he was actually on that part of the coast, though, from another letter addressed to the President of the Royal Society of London, it might be conjectured that his information was obtained from observation on the spot. 'No land,' he says, 'is considered to exist to the northward of it. The east side of the Noss is composed of bold and perpendicular bluffs, while the west side exhibits gradual declivities; the whole most sterile, but presenting an awfully magnificent appearance.' From the Kolyma to Okotsk, he had, he says, a 'dangerous, difficult, and fatiguing journey of three thousand versts,' a great part of which he performed, on foot, in seventy days. After such an adventurous expedition from Petersburgh, to the north-eastern extremity of Siberia, we regret to find that the shores of Kamschatka are likely to be the boundary of his arduous and perilous enterprise. After gratefully noticing the generosity and consideration which he every where experienced at the hands of the Russian Government and of individuals, he adds—'that Government has an expedition in Behring's Straits, whose object is to trace the continent of America to the northward and eastward. I had the same thing previously in view: but it would be vanity and presumption in me to attempt a task of the kind, while their

means are so much superior, and those who are employed on it, *authorized* travellers. Thus circumstanced, it can create no surprise that an humble individual, like myself, should submit to make a sacrifice of private gratification, and every prospect of success, to a sense of the impropriety of proceeding further at present, and of the indelicacy which would result from such a step; but, should the commander of the expedition, from any circumstances, desist from the further prosecution of his discoveries, *I shall, in that case, continue my journey eastward* '— the meaning of all which will, we think, be perfectly intelligible, from what we are about to state.

The expedition noticed by Captain Cochrane consisted of two ship corvettes which left Spithead in the year 1819, at the same time that the expedition alluded to in our first paragraph proceeded to the southern hemisphere. In July 1820, they reached Behring's Strait, and were supposed to have passed it in that year; they returned, however, in the winter to some of the Russian settlements on the coast of America; and, as now appears from Captain Cochrane's letter to us, were again in that neighbourhood in June 1821: of their ulterior proceedings no intelligence had reached Petersburgh at the period of the latest accounts from that capital. If they should have succeeded in doubling Icy Cape, it is just possible that they may fall in with Captain Parry, provided they are lucky enough to escape the fate of Sir Hugh Willoughby and his unfortunate associates: of such a catastrophe, we are by no means sure that they do not run a very considerable risk, from the slight and insufficient manner in which they were fitted out; being, in fact, destitute of every necessary for passing a winter in the Frozen Ocean, and, as we happen to know, in want even of the common implements for encountering the ice: with some of the latter, however, they were supplied from the dock-yard of Portsmouth, on application to the British Government.

We should not be disposed to detract from the merit which, in this instance, would be justly due to the Russian government, if we could persuade ourselves that the extension of geographical knowledge, for its own sake and the benefit of mankind, was the prime object of this expedition; but when we couple it with the cautious language of Captain Cochrane, and the sudden and unexpected check thrown in the way of his further progress, after reaching the shores of Behring's Strait, and also with a contemporaneous ukase of a most extraordinary nature (if we may credit what appears in the public journals), we cannot but entertain some suspicion, that His Imperial Majesty, in his northern expeditions, has been governed by other motives than those of merely advancing the cause of science and discovery.

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In this curious manifesto (for such in effect it is) the maritime powers of Europe and America are given to understand that His Imperial Majesty of Russia has assumed possession of all that portion of the north-west coast of America, which lies between the fifty-first degree of latitude and the Icy Cape, or extreme north; and moreover, that he interdicts the approach of ships of every other nation to any part of this line nearer than one hundred miles. Whether this wholesale usurpation of 2000 miles of sea-coast, to the greater part of which Russia can have no possible claim, will be tacitly passed over by England, Spain, and the United States, the three powers most interested in it, we pretend not to know; but we can scarcely be mistaken in predicting that His Imperial Majesty will discover, at no distant period, that he has assumed an authority, and asserted a principle, which he will hardly be permitted to exercise; and that there is an ancient common law of nations which will not, and cannot, be abrogated by the '*sic volo*' of a power of yesterday. It has apparently escaped the recollection of His Imperial Majesty's advisers, that if his example were to be followed by the maritime nations of Europe, his own ports would be hermetically sealed, and an end put at once to the assumption of long appropriated coasts by Russia.

With respect to the legality of taking possession of an unoccupied territory, to the exclusion of the original discoverer, some doubts, we understand, are still entertained among jurists. It is time, we think, to come to a decision one way or another, on a point of so much importance. Let us examine, however, what claim Russia can reasonably set up to the territory in question. To the two shores of Behring's Strait, we admit, she would have an undoubted claim, on the score of priority of discovery; that on the side of Asia having been coasted by Deshnew in 1648, and that of America visited by Behring in 1741, as far down as the latitude 59°, and the peaked mountain, since generally known by the name of Cape Fairweather: to the southward of this point, however, Russia has not the slightest claim. The Spaniards visited the northern parts of this coast in 1774, when Don Juan Perez, in the corvette Santiago, traced it from latitude 53° 53′ to a promontory in latitude 55°, to which he gave the name of Santa Margarita, being the north-west extremity of Queen Charlotte's Island of our charts; and on his return, touched at Nootka, about which we were once on the point of going to war. In the following year, the Santiago and Felicidad, under the orders of Don Juan Bruno Heceta, and Don Juan de la Bodega y Quadra, proceeded along the north-west coast, and described, in latitude 56° 8′, high mountains covered with snow, which they named Jacinto; and also a lofty cape, in latitude 57° 2′, to which

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they gave the name of Engaño. Holding a northerly course, they reached lat. 57° 58′, and then returned.

Three years after these Spanish voyages, Cook reconnoitred this coast more closely, and proceeded as high up as the Icy Cape; it was subsequently visited by several English ships for the purposes of trade; and though every portion of it was explored with the greatest accuracy by that most excellent and persevering navigator, Vancouver, as far as the head of Cook's Inlet, in lat. 61° 15′; yet, on the ground of priority of discovery, it is sufficiently clear that England has no claim to territorial possession. On this principle, it would jointly belong to Russia and Spain; but, on the same principle, Russia would be completely excluded from any portion of it to the southward of 59°. She has, however, been tacitly permitted to form an establishment, named Sitka, at the head of Norfolk Sound, in lat. 57°; and this, apparently, must have tempted her to presume, that no opposition would be offered to an extension of territory down to the fifty-first degree of latitude, which includes all the detailed discoveries of Cook and Vancouver, *i. e.* New Hanover, New Cornwall, New Norfolk on the main, and the Islands of King George, Queen Charlotte, and Prince of Wales upon the coast.

There is, however, one trifling circumstance, of which we are persuaded His Imperial Majesty was ignorant when he issued his sweeping ukase, namely—that the whole country, from lat. 56° 30′ to the boundary of the United States in lat. 48°, or thereabouts, is now, and has long been, in the actual possession of the British North-west Company. The communication with this vast territory is by the Peace River, which, crossing the Rocky Mountains from the westward, in lat. N. 56°, and long. 121° W., falls into the Polar Sea by the Mackenzie River. The country behind them, to the westward, has been named by the settlers New Caledonia, and is in extent, from north to south, about 500 miles, and from east to west 300 miles. It is described as very beautiful, abounding in fine forests, rivers, and magnificent lakes, one of which is not less than 300 miles in circumference, surrounded by picturesque mountains, clothed to their very summits with timber trees of the largest dimensions. From this lake, a river falls to the westward into the Pacific, either into Port Essington, or Observatory Inlet, where Vancouver discovered the mouths of two rivers, one in lat. 54° 15′, the other in 54° 59′. In the summer season, it swarms with salmon, from which the natives derive a considerable part of their subsistence. The North-west Company have a post on its borders, in lat. 54° 30′ N., long. 125° W., distant about 180 miles from the 'Observatory Inlet' of Vancouver, the head of which lies in lat. 35° 15′ N. long.

long. 129° 44′ W., where, by this time, the United Company of the North-west and Hudson's Bay have, in all probability, formed an establishment, and thus opened a direct communication between the Atlantic and the Pacific, the whole way by water, with the exception of a very few miles across the high lands, which divide the sources of the rivers, and give them opposite directions.

Thus then it is obvious, that, as we have actual possession of the six degrees of coast usurped by Russia in her recent manifesto, her claim to this part is perfectly nugatory. Indeed, as we before observed, the assumption must have been made in utter ignorance of the fact; which is the less surprising, as this part of the world remains, as yet, a complete blank on our best and latest charts.—*Quarterly Rev.* No. 52.

ARCTIC EXPEDITION.

On the 9th of March last, at six o'clock A.M., a countryman who was employed gathering sea-weed on the Irish shore, in the parish of Clonmauny, county of Donegal, found a bottle which had been thrown out by His Majesty's ship Fury, Captain Parry, in lat. 62. 8. N., long. 62. 27. W. The countryman, anxious to ascertain the contents of the bottle, conceiving it contained something which might be valuable to him, instantly broke it, and found a paper, on which was inserted the following in seven languages:—

"His Majesty's ship Fury.—Set off July, 1821, lat. 62. 8. N. long. 62. 27. W. At one, P.M. moderate breezes, from the Northward, dull misty weather. Hecla in company.

"W. PARRY, Commander."

This paper he gave to Mr. Chichester, who immediately transmitted it to the Admiralty. The shore where the bottle was found is in lat. 55. 15. N., long. 7. 28 W.

DISCOVERIES IN EGYPT.

M. Acerbi, the Editor of the *Biblioteca Italiana*, in the number for March, gives an extract of a letter from M. Zuccoli, dated Sennaar, Nov. 3, 1821.

M. Z. accompanies the army of Ibrahim Pacha, son of the Viceroy of Egypt, as officer of engineers, and is charged with the geographical survey of the countries through which it passes. When the letter was dispatched the army was in 13° north latitude, and was to advance to the 7th degree. In that variable climate a heat from 31 to 35 degrees of Reaumur by day, with a coolness of 15° by night, causes frequent diseases.

M. Zuccoli has made a survey of the Nile from Alexandria to Sennaar. He counted 180 more or less considerable cataracts in

in the Nile, which were all passed, however, with very small loss either of vessels or people. He remarks an error in Bruce's map. Bruce makes the Dender fall into the Rahb, and the latter into the Nile; whereas both these rivers fall into the Nile, the Dender fifteen miles above the Rahb. Where Bruce wrote according to the information of the inhabitants, and did not see with his own eyes, no confidence can be placed in him; for the people, says M. Zuccoli, are so ignorant that they hardly know where the sun rises and sets. They cannot distinguish north from south. He thinks he has found the island of Meroe in the slip of land between the Dender and the Rahb, where he discovered 45 pyramids covered with hieroglyphics. He met here with M. Caillaud and his companion, who followed another army under Ismail Pacha, another son of the Viceroy's. He waited for the armed vessels, to proceed as far as possible up the White River, and see whether it comes, as is said, from a great inland lake, and is connected with the Niger, or at least is in its neighbourhood.—*Allgemeine Zeitung, May* 8.

EARTHQUAKES.

The shock of an earthquake was very distinctly felt at Crieff and neighbourhood, betwixt 9 and 10 o'clock on the morning of the 18th inst. The shock was so severe at Ferntower, the seat of Sir David Baird, as to set the bells of the house a-ringing.——*Stirling Journal.*

Extract of a Letter, dated Comrie, 15th of April:—"About half past nine on Saturday (the 13th inst.), while at breakfast, we were visited with the smartest shock of an earthquake that has been felt in this neighbourhood for the last fifteen or twenty years. It was accompanied by two very loud reports, one apparently above our heads, and the other, which followed immediately afterwards, under our feet. The noise of these, which were much more terrific than thunder, lasted, I should think, fully thirty seconds. It set our kitchen utensils a-ringing, and brought down some of the covers of the pots and pans. I have felt much severer shocks in the West Indies, but not accompanied with such a noise. The sensation it created in me was exactly like that I have felt on the deck of a vessel on her guns being discharged."

The Neapolitan Journals announce, that on the 22d of March two immense openings of the earth took place on the sea-shore of Marsala, in Sicily. The same day a vessel was thrown amongst rocks by an extraordinary motion of the waves, though the sea, only a few moments before, was perfectly tranquil. It was supposed that these phenomena were produced by a submarine volcanic eruption.

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METEORS.

On the 16th of March, about five minutes after ten o'clock P. M. a meteor of a most extraordinary size and brilliancy passed over the city of Richmond (Virginia), in a direction from the north-east to south-west. It is represented by persons who saw it, as nearly the size of a barrel; that sparks were emitted from it in every direction; and that it left behind it a trail of light of great length; and it was thought by some that they heard a hissing noise as it passed over them. By persons who saw it, it is described as emitting a silver light, more bright, if possible, than the sun. It exploded with a loud noise, leaving behind it a wide stream of fire, which was visible for some minutes.

A letter from Rodez, in Aveynon, says, "That on the 9th of April, about nine o'clock in the evening, a grand and beautiful meteor was observed in that town. It appeared in the form of a pillar of fire, sending forth a dazzling light like that of the sun. From this luminous body there issued, as from artificial fire-works, an infinite number of sparks of fire. After some seconds the meteor disappeared, and at the same moment a loud explosion was heard."

LIST OF PATENTS FOR NEW INVENTIONS.

To Pierre Erard, of Great Marlborough-street, musical-instrument maker, who, in consequence of communications made to him by a certain foreigner residing abroad and discoveries by himself, is in possession of an invention of certain improvements on harps.—Dated 24th April 1822.—6 months allowed to enrol specification.

To Edward Dodd, of St. Martin's lane, Middlesex, musical-instrument maker, for certain improvements in pedal harps.—24th April.—6 months.

To James Delveau, of Wardour-street, Middlesex, musical-instrument maker, for certain improvements on harps.—24th April.—2 months.

To Robert Ford, of Abingdon-row, Middlesex, chemist, for his discovered liquid or solution of annotto.—24th April.—2 months.

To Robert Knight, of Foster-lane, Cheapside, London, ironmonger; and Rupert Kirk, of Osborne-place, Whitechapel, Middlesex, dyer, for their process for the more rapid crystallization and for the evaporation of fluids at comparative low temperatures.—9th May.—2 months.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND.

Days of Month. 1822.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Weather.
April 27	50	55	50	30·04	Rain
28	50	61	50	·29	Cloudy
29	50	63	50	·28	Fair
30	55	65	50	·30	Fair
May 1	53	62	48	·35	Fair
2	48	62	50	·14	Fair
3	45	63	52	29·90	Fair
4	57	64	54	·65	Cloudy
5	50	67	57	·74	Thunder
6	57	67	57	·77	Cloudy
7	55	55	50	·84	Rain
8	45	54	44	·97	Fair
9	44	52	46	·69	Cloudy
0	50	54	45	·32	Rain
11	49	61	50	·66	Fair
12	48	48	46	·82	Cloudy
13	49	55	48	·86	Cloudy
14	50	65	52	·90	Fair
15	53	68	50	·94	Fair
16	50	64	51	30·00	Fair
17	55	71	56	29·99	Fair
18	56	73	51	30·07	Fair
19	58	73	60	·13	Fair
20	62	75	63	·18	Fair
21	60	72	62	·33	Fair
22	55	69	50	·37	Fair
23	5[illegible]	62	49	·27	Fair
24	55				

N B The Barometer's height is taken at one o'clock.

LXXX. *On the Graduation of the Pantograph.* By J. W. Woollgar, *Esq. Lewes.*

To Dr. Tilloch.

Sir, — The instrument called the Pantograph is so well known, that a minute description of it may here be dispensed with. The principle of its operation depends upon the geometrical doctrine of similar triangles; but the mechanical construction has undergone several modifications. In its early form, the fulcrum, pencil, and tracing point, occupied fixed positions on the bars, and the ratio of reduction, that is, the proportion of the original draught to the copy, was determined by varying the situation of the pivots. It was afterwards found that greater mechanical accuracy could be attained by causing the fulcrum and pencil to shift their positions on two of the bars, while the pivots remained invariable. To this latter construction, as it is represented on Plate 31 of Adams's Geometrical and Graphical Essays, and as it is to be met with in the shops of the London makers, the present remarks are intended to apply.

On the two left-hand bars, which carry the sliding tubes appropriated to the fulcrum and pencil, are usually engraved eleven transverse lines, marked $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, and so on to $\frac{1}{12}$. By adjusting the sliding tubes to the corresponding marks on each bar, the pencil being attached to the short bar, the instrument will produce a copy whose scale will be an aliquot part of the original, as indicated by the engraved fractional number. And if the pencil be attached to the lowermost division (marked $\frac{1}{2}$) on the long bar, and the fulcrum occupy a corresponding place on the short bar, a copy will be traced of the same size with the original.

So far, *and no further*, are we instructed in the uses of this important instrument, by Mr. Adams's work above quoted, and by the other books which I have consulted. But there is an indefinite number of other proportions, which a copy may be required to bear to its original, between the ratio of equality and that of 1 : 2, and between this last and that of 1 : 12; and in fact by far the greater number of cases occur, in which the required scale of the copy is *not* an aliquot part of the original. How then is the instrument in these instances to be adjusted so as to produce the desired effect?

"There are sometimes," says Mr. Adams, "divisions of 100 *unequal* parts laid down on the bars, to give any intermediate proportion, not shown by the fractional numbers commonly placed." This is exactly what is wanted; but then such divisions must be *actually* laid down on the instrument, and they

must be *properly* done. I have not seen a sufficient number of the larger instruments to be able to speak positively on this point, but I believe that in general the above requisites are not complied with.

In a two-foot instrument of excellent workmanship, which now lies before me, there is a scale of 100 parts on each bar; that is, the space between the extreme marks $\frac{1}{2}$ and $\frac{1}{12}$ is divided into 100 *equal* parts, zero being against the upper end. The want of principle manifested by this arrangement is obvious; for it may be demonstrated that, whether the pencil be attached to the long or to the short bar, that scale only which is adapted to it ought to contain equal divisions; and the other, or scale for the fulcrum, must be unequally graduated. The scales, therefore, on the instrument alluded to, are worse than useless, for they may mislead the practitioner, in case he should forget the fundamental rule, that *in all cases the three operative points must be in a right line.*

When the ratio of reduction is greater than $\frac{1}{2}$, the pencil must be attached to the long bar, and the fulcrum to the short one. Again, when the ratio is not greater than $\frac{1}{2}$, it will be proper that the pencil and fulcrum should change places. Each of these cases requires a distinct scale for each bar.

The scales applicable to the first case, I shall call A, and their divisions will be numbered in each bar from 100 to 50. Those applicable to the second case, I shall call B, and they will be numbered from 50 down to 8, or lower, if the mechanism will admit of it, though a less ratio than 100 to 8 will rarely be wanted in practice.

Let the ratio of the original to the copy be represented by $1 : r$, and let $2\,d =$ the *working length* of the instrument, that is, the distance from the vertical pivot to the centre of the tube that carries the tracer, or to the lowest mark on the left-hand bar: consequently $d =$ the distance between the two pivots on the last-mentioned bar, and equal to the distance from the pivot at the upper end of the short graduated bar to the lowest mark on it. Then the distances of the several divisions, to be laid off from the pivot connecting the graduated bars, will be as follows:

For the scale A $\begin{cases} d\,r \text{ on the long bar.} \\ \dfrac{2\,d\,r}{r+1} \text{ on the short bar.} \end{cases}$

For the scale B $\begin{cases} \dfrac{d\,r}{1-r} \text{ on the long bar.} \\ 2\,d\,r \text{ on the short bar.} \end{cases}$

According to the above formulæ the subjoined table is constructed, assuming $2\,d = 1$; consequently, to apply the table

to

to any particular instrument, the number on it must be multiplied by the actual value of $2d$.

Such is the facility and extension given to a pantograph when graduated as above described, that I would recommend the possessors of well-made instruments which are defective in this respect, to have the scales properly laid down upon them. But if they do not choose to be at that expense, they may avail themselves of the formulæ and table here given, to lay down marks on their bars as occasion may require; and in such case it would be a preferable mode, instead of the tabular value, to take their complements to 0·5, and to set off the distances from the lowermost division on the instrument, which is marked $\frac{1}{2}$.

It may be remarked, that the expression $\frac{2dr}{r+1}$ is applicable to the graduation of the proportional compasses, $2d$ being the entire length of the instrument. The distance between the centre of the joint and the index mark is to be added, as a constant quantity to all the values. I am, &c.

Lewes, April 16, 1822. J. W. Woollgar.

Scale A.			Scale A.			Scale B.		
Div.	Long B.	Short B.	Div.	Long B.	Short B.	Div.	Long B.	Short B.
100	0·50	0·5000	67	0·335	0·4012	40	0·3333	0·40
99	·495	·4975	66	·33	·3976	39	·3197	·39
98	·49	·4949	65	·325	·3939	38	·3065	·38
97	·485	·4924	64	·32	·3902	37	·2937	·37
96	·48	·4898	63	·315	·3865	36	·2812	·36
95	·475	·4872	62	·31	·3827	35	·2692	·35
94	·47	·4845	61	·305	·3789	34	·2576	·34
93	·465	·4819	60	·30	·3750	33	·2463	·33
92	·46	·4792	59	·295	·3711	32	·2353	·32
91	·455	·4764	58	·29	·3671	31	·2246	·31
90	·45	·4737	57	·285	·3631	30	·2143	·30
89	·445	·4709	56	·28	·3590	29	·2042	·29
88	·44	·4681	55	·275	·3548	28	·1944	·28
87	·435	·4652	54	·27	·3506	27	·1849	·27
86	·43	·4624	53	·265	·3464	26	·1756	·26
85	·425	·4595	52	·26	·3421	25	·1667	·25
84	·42	·4565	51	·255	·3377	24	·1579	·24
83	·415	·4536	50	·25	·3333	23	·1494	·23
82	·41	·4505				22	·1410	·22
81	·405	·4475	Scale B.			21	·1329	·21
80	·40	·4444				20	·1250	·20
79	·395	·4413	Div.	Long B.	Short B.	19	·1173	·19
78	·39	·4382				18	·1098	·18
77	·385	·4350	50	0·5000	0·50	17	·1024	·17
76	·38	·4318	49	·4804	·49	16	·0952	·16
75	·375	·4286	48	·4615	·48	15	·0882	·15
74	·37	·4253	47	·4434	·47	14	·0814	·14
73	·365	·4220	46	·4259	·46	13	·0747	·13
72	·36	·4186	45	·4091	·45	12	·0682	·12
[illegible]	·355	·4152	44	·3929	·44	11	·0618	·11
70	·35	·4118	43	·3772	·43	10	·0556	·10
69	·345	·4083	42	·3621	·42	9	·0495	·09
68	·34	·4048	41	·3475	·41	8	·0135	·08

LXXXI. *On the Porcelain Clay and Buhr-stone of Halkin Mountain, Flintshire.* *By* W. Bishop *and Co., of Nant y Moch, near Holywell in that County*.*

The qualities which fit a stone for grinding corn, especially wheat, are hardness, to prevent it as much as possible from wearing down by the constant friction to which it is exposed, a certain degree of tenacity, to prevent the grinding surface from scaling or chipping off, and a cellular structure, in order to increase the quantity of cutting surface, the walls of the cells being at the same time thick enough to resist the strain upon them.

The advantages hence resulting are, that the flour is in no material degree contaminated by the mixture of earthy particles abraded from the stones, the grinding is expeditiously performed, the bran is completely disengaged from the flour, and the flour itself is very little heated by passing through the mill. This latter circumstance is of great importance, it being found, by experience, that flour over-heated, or *killed*, as the technical phrase is, will never produce bread so light as that which is ground cool.

In some parts of the valley of the Seine and of the adjoining districts in which the fresh-water limestone occurs, is found a siliceous rock, in detached masses or blocks, of various size, known on the spot and in commerce by the name of *buhr*. It is a substance intermediate between hornstone and calcedony, and possesses, in an eminent degree, the qualities which peculiarly fit it for grinding wheat. All the fine flour required for the supply of the metropolis and of the other large towns in this island is prepared by means of millstones of French buhr, a circumstance which, beside rendering us dependent on foreigners for so essential an article, is the occasion in time of war of enormously enhancing the price, and subjecting our millers to great inconvenience.

The northern shore of the Isle of Wight is the only district in this country in which the fresh-water limestone has hitherto been found, but it does not appear to contain any buhr-stone. The entrochital chert or hornstone (vulgarly called *screwstone*) which occurs interstratified with the mountain limestone in Derbyshire, as it resembles buhr-stone in quality and texture, has occasionally been made trial of for a grinding-stone, but always unsuccessfully on account of its fragility and softness.—*Sec.*

In the year 1816 Mr. Thomas Hooson, of Flint, observed on

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1821. The Society's Isis gold medal was voted for this communication.

Halkin

Halkin mountain a bed of remarkably fine porcelain clay, which, on exposure to the potters' fires, was found to assume a more delicate whiteness than any substance of a similar nature hitherto found in this kingdom; and seeing also other substances which he thought likely to be useful to the potters, he obtained from Earl Grosvenor a lease of all clays, rocks, and stones (except limestone) within his lordship's liberties; and subsequently, with a view to an extended trade, formed his present partnership with Mr. Richard Fynney, Mr. William Bishop, and Mr. James Whitehead, established under the firm of the "Welsh Company at Nant y Moch, near Holywell," where they have erected works for preparing the clay, which is called "Cambria," for sale, by separating it from a white siliceous sand and rock, with which the bed is found mixed to a depth at present unknown, but which has been proved as deep as 26 yards. The sand, when separated, is used for glass-making; and the white siliceous rock, now called "Rock Cambria," is ground down and used in the composition of china and earthenware, instead of ground flint, or is mixed with it. For this process of grinding, several thousand tons of chert are annually consumed in the Staffordshire potteries, and much is supplied from Halkin mountain. In quarrying this chert, some of it in the state of vesicular entrochital horn-stone was raised, which, when used together with common chert, indicated such a superiority by its expeditious grinding and its little wear, and showed such a proximity in appearance (after having been worked) to the French buhr, that its use for grinding wheat was considered probable; and this led to the first application of the vesicular Halkin rock as a buhr-stone.

Halkin Mountain (called "Alchene" at the Conquest, according to Pennant) is a range of high uncultivated land in Flintshire, the mineral property of the right honourable Earl Grosvenor. On the inland side it runs parallel to the boundary hills of the vale of Clwyd; and on the north-east stretches from Holywell for about four miles till nearly opposite Northop, in an angle of about twelve degrees with the river Dee, and averages about a mile in breadth. It is composed of mountain limestone, with the usually accompanying rocks, and abounds with large veins containing lead ore, blende, and calamine, with some appearances of copper; it also affords a rock of a whitish quartz, well adapted for certain kinds of mill-stones, for which (according to all our old historians) Flintshire has been famous. But these quarries had been neglected for many years, till lately reopened by the discoverers of the still more valuable buhrs, and promise to regain their celebrity as gray stones for grinding oats, &c.

The

The buhr-stone itself, or entrochital horn-stone, is found near the middle of the eastern ridge of Halkin mountain, and on the west side of the ridge, into which it penetrates with a dip of about one yard in six. Its present appearance presents a bed of about four yards thick, between two layers of a compact siliceous slaty chert, covered with a shivery siliceous shale. It dips eastwardly, like all the other strata on the mountain, which consist of limestone rock and chert. The buhr-stratum is principally of the same quality as the small mill-stone sent herewith, and attested by Dr. Traill (Certificate, No. 1); but rotten masses sometimes occur, and blocks are occasionally found of too close a texture for the miller; and some few are quite solid. Still the corallite structure pervades the whole: the entrochites being perfect and entire in some instances, while in the chief parts of the bed the casts alone remain; thus leaving the rock vesicular, and in this respect differing from the nature of the pores in the French buhr, which appear to have been caused by corrosion, their edges being rusty and impure, whereas those in the Halkin buhrs are of pure flint, and exceedingly sharp and hard.

The quarry from which all the buhrs hitherto used have been procured, now presents a fore-breast of forty yards, and is of the same quality and thickness as at first, but has a thicker covering of shale as it dips into the hill. At the distance of a mile to the north-west, a second quarry is now opening, and appears similar in every respect to the former; and from fragments of buhr here and there found, with pieces of shale and of chert, half concealed in the mountain turf, traces of the same stratum may be observed from the one quarry to the other. About half a mile to the south-east of the main quarry, in the same chert-formation, the buhr-stone is also seen to crop out; and in the valley at the foot of the ridge, where a thick bed of limestone forms the upper stratum, with a sub-stratum of chert, the miners, in their search for lead-ore, have met with the buhr-stone at the depth of 160 yards from the surface.

In order to prove the Halkin buhrs, the discoverers had some made into mill-stones, which they set up in a neighbouring mill in the borough of Flint; some were had by a mill-wright, and afterwards sent to a mill at Dunham-o'-th'-Hill, mixed with French buhrs; and one large buhr was shaped into a mill-stone, and put up at a mill at Ysceifiog.

They considered it would require much time to prove the real character of the buhrs, and that it would be useless to endeavour to make sales till this proof could be satisfactorily made, and therefore they took but little trouble in circulating the object of their discovery for nearly two years, when finding that the

the stones at Flint mill were highly approved, and found to be a substitute for the French buhrs, they turned their attention to the subject.

They were advised to lay specimens of the buhrs before the Society of Arts, &c. immediately, lest they might be anticipated by some other person in their pretensions to the premium offered, and they accordingly ventured to do so, in February 1820 (under the name of Flint Buhrs); but not having then had sufficient trial made of them, they were not in possession of certificates sufficiently extensive on which to rest their claims to the notice of the Society.

As, however, they are now able to adduce proofs that the Halkin buhrs are fully equal to the French, and in some cases are declared to be actually superior to them, they trust that the Society, in looking to the national importance of the discovery, will pass over the trouble that was last year so unintentionally occasioned, and again take the matter into their consideration.

They request permission to lay before the Society the accompanying certificates and letters on the subject; and in order to show that they have not been selecting a few, and withholding any less favourable to their hopes, they beg to state the result of every sale made by them up to the end of the last year, and to add a short review of the particular certificate connected with each case, observing at the same time that not one unfavourable or unsatisfactory trial has yet occurred.

Some of the buhrs got on the discovery of the quarry were (as before stated) converted into mill-stones, and put up about three years ago at Mr. Evans's mill, in the borough of Flint, who certifies that "he used them nearly two years occasionally for wheat, but chiefly as gray-stones, in which they excelled; that at first he used them seldom for wheat, but afterwards more and more frequently, as he found them answer the purpose; and, by way of comparing them with the French stones, he took six measures of wheat, and ground one-half on the Halkin stones, and one-half on the French stones; there was some very slight difference in the flour, which was in favour of the French; but he did not consider it as a fair trial, as the Halkin stones were not at the time properly faced for wheat grinding, and if the French stones had been faced as rough, the flour from them would not have been better than the other. Bread was made from the two kinds of flour, but no one could distinguish between the two. He then had the Halkin stones regularly faced and cracked as French, and has found them ever since equal to the French stones in every respect whatever."

Others of the buhrs, got about the same time, were used more cautiously by a millwright, who made a large pair of millstones

of

of Halkin and French buhrs, fixed in alternately, and these were set up more than three years ago at the Hornmills, near Dunham-o'-th'-Hill, in Cheshire. Mr. John Peers, the present tenant of this mill, entered on it nearly three years ago, and he states that "the stones were in a rough state, and required six months to get them to a proper face, when they ground wheat as well as the best French stones, and have ever since continued to do so; that he prefers the Halkin and French stones (mixed) to those of French buhrs entirely, as they grind faster, and as well, and full as cool as the French; that he uses them for all purposes, and considers them equal in every respect, and superior in some respects to the French buhrs."

A large buhr got about the same time, was sold to Mr. John Edwards, the occupier of a small mill at Ysceifiog, in Flintshire; he states "that from various causes the buhr was not used till about twelve months ago, when he shaped it into a millstone of three feet six inches diameter; that he has no French stones, but used this as a runner over a blue stone for grinding wheat, and found the flour of good colour, and the bran broad and light; that the stone would bear the finest cracking, and continued to improve and harden till he left the mill in November last."

The next sale was to John Dumbell, Esq., of the Mersey Mills, Warrington (said to be the largest establishment in the kingdom, and containing twenty-two pair of mill-stones), and he certifies that "in March 1820 he received a quantity of Halkin buhrs, which he had forthwith made into mill-stones, and these were so much approved, that in May 1820 he had buhrs for a second pair; that the two pair of Halkin millstones had been regularly at work ever since, and continue to give great satisfaction to the bakers and flour-dealers; that he conceives they are precisely the same kind of stone as the French buhrs, and cut the grain like them, and are like them in respect to oatmeal, in which neither French nor Halkin stones are used to advantage: and he considers the discovery of great national importance." Messrs. Hurstfield and Passand (now the occupiers of some large mills at Lymm, near Warrington, but who were lately foremen to Mr. Dumbell, and have been practical millers nearly thirty years) state "that they made the Halkin stones which were set up at the Mersey Mills, where there are nine pair of French stones at work; that they made an experiment with some wheat, by grinding some on the best French pair, and some on the Halkin stones, in order to compare the flour, in which there was scarcely any perceptible difference, though the preference was given in favour of the Halkin stones by a corn- and flour-dealer to whom the samples were shown; that bread was made from each, but no difference could be perceived; that, at first they thought the Halkin stones not quite

so

so hard and tough as the French, but they found them continue to improve, and to become as good as French; that they have seen all varieties of millstones, and made all sorts of millstones, but never saw any buhrs to come in competition with the French, except the Halkins, which they are satisfied will answer every purpose."

In corroboration of these statements, a sample of the bran (sifted in its rough state out of the flour) is respectfully submitted to the Society.

In May 1820 a Halkin millstone was sent to Mr. Pratt, of Saredon Mill (a large concern near Walsall, in Staffordshire), and set to work in his mill at Dudley. Mr. Pratt has had a very extensive practical experience for more than thirty years, and in October last he wrote that "it had been applied for several weeks in grinding wheat, and that it ground equal to French stones, and better than some of them; but he had it for grinding barley, &c., and was so using it, and found it answer remarkably well for that purpose; that the face and dress keep good, and for a great length of time; and that in the spring he would have a pair of Halkin stones to grind wheat." Upon application to Mr. Pratt for the result of his further experience, he writes again on the 26th February, that "he gave a just report of the good qualities of our Halkin millstone in October last, and entertains the same opinion to the present day; but that it had been grinding barley, &c. ever since, and he never before met with any stones to bear hard grinding so well, and continue the dress so long."

In June 1820, Mr. Stephens, the owner and occupier of a steam mill in Harrington, Liverpool, having a desire to try the Halkin buhrs, obtained a buhr, which he broke into several pieces, and fixed them into different parts of a pair of French buhr millstones; and he certifies, that "they have since worked to a good face, and crack as well as the rest of the stones; and as far as his opinion can be formed by such a circumstance, he considers the Halkin equal to the French buhrs." He states also, "that he has, at the request of the discoverers, taken out one of the pieces of Halkin buhr from his millstones," which they beg to offer to the attention of the Society as a convincing proof of the toughness and hardness they manifest after a few months' wear, being in this respect also like the French buhrs.

In August 1820, a pair of Halkin millstones, of five feet diameter, were sent to Messrs. Pilling and Co.'s large mills, at Mirfield, near Leeds, who have not yet given any written report of the stones; but Mr. Goodwin of Liverpool (a mutual friend of Mr. Pilling and of the proprietors of the quarry), states, that he lately had a conversation on the subject at Mirfield with Mr.

 Pilling,

Pilling, who stated "that the stones were not quite so uniformly porous as the sample buhr, and had rather chipped in facing; that they mended of this every time they were faced, and were evidently tougher the longer they worked."

[N. B. It is intended to send up Mr. Pilling's own report, by way of supplement, as soon as it can be procured]

In September 1820, a pair of Halkin millstones was put up at the Aughton water-mill, near Ormskirk, Lancashire, occupied by Mr. Richard Rawsthorn, sen., who has been a practical miller all his life, and is 74 years old, and he states, that "they answer better than French, for they grind cool, and make fine flour, cut bran thin and broad, and crack as fine as any French stone."

In September 1820, a Halkin millstone was also put up at a new windmill at Knotty Ash, near Liverpool, and Messrs. Marr, the tenants, declare that "they laid down a pair of French buhrs, and a short time after laid down a French and Halkin; that the latter work equally well as the French; stand cracking as well, have been dressed four times. and still improve; soften the wheat as well or better than French do, and cut very broad bran, and preserve the colour as well as any French stone."

In October 1820, a pair of Halkin millstones were sent to Messrs. Hudson and Co., of the King's Mills, Leeds. By a letter from them it appears the stones are not yet in use, so that no positive proof can be had of their grinding; but they say "that their millers who have prepared the stones for work (from which they can form a good opinion of their qualities in comparison with French buhrs) give them a favourable opinion that they are likely to answer."

In November 1820, a pair of Halkin millstones were consigned to Richard Robinson, Esq. of the Phœnix Iron Works, Dublin; but they were delayed for a long while by stress of weather, and have not yet been put to work. Mr. Robinson, however, says that "they have undergone a very close examination by some of the first millwrights and millers, who all agree that they appear equal to the French buhrs, and in some instances superior," alluding (it is supposed) to the equability of the pores.

In December last, Richard Sankey, Esq., banker in Holywell, Flintshire, and owner of a large windmill there, having a pair of French millstones which did not give entire satisfaction, removed the runner, and put up a Halkin millstone in lieu of it, and he certifies, that "his tenants like the work done by these better than by the other pair of French stones in the mill; that they clean the bran better, that the flour is soft and of good colour, and the stone keeps its face well, and gives satisfaction in all respects."

The discoverers beg permission to declare further (and are ready

ready to do so on their oath if desired), that the several certificates above mentioned have been given voluntarily and gratuitously, and that the several persons giving them have no concern or interest in the quarry; and that up to the end of the last year, no Halkin buhrs or millstones have been disposed of in any instance except those before mentioned; namely,

Mr. Edward Evans, Flint Mill, Flintshire.

Mr. Peers, Horn Mills, Dunham-o'-th'-Hill, near Overton, Cheshire.

Mr. John Edwards, Ysceifoig, Flintshire.

John Dumbell, Esq., Mersey Mills, Warrington, Lancashire.

Mr. Pratt, Saredon Mill, near Walsall, Staffordshire.

Mr. Stephens, Steam Mill, Hill-street, Harrington, Liverpool.

Messrs. Pilling and Co., Mirfield Mills, near Dewsbury, Yorkshire.

Messrs. Hudson and Co., King's Mills, near Leeds, Yorkshire.

Mr. Rawsthorn, Aughton Water-Mill, near Ormskirk, Lancashire.

Mr. Marr, Knotty Ash Windmill, near Liverpool.

Richard Robinson, Esq., Phœnix Iron Works, Dublin.

Richard Sankey, Esq., Banker, Holywell, Flintshire.

They have therefore offered to the Society all the evidence which it is possible to produce, and trust that when the various testimonials (collected from different sources and from persons who have had no communication with each other, though all agreeing in approbation) shall have been compared, the Society will be pleased to honour the Halkin Buhrs with their sanction.

W. Bishop & Co.

The several samples alluded to in the preceding Report are placed in the Repository of the Society.

Certificates

from all the persons named in the preceding statement accompanied the communication of Messrs. Bishop and Co.; of which the following, as being the most important, are subjoined:

No. I.

Liverpool, March 3, 1821.

I have this day examined the small millstone, of Flint buhrstone, measuring $11\frac{1}{2}$ inches in diameter, which is about to be sent to London for the inspection of the Society of Arts, and hereby certify, that it is a fair specimen of the rock in the quarry on Halkin Mountain, which I visited last year; a vast quantity of stone, of a quality equally excellent with this specimen, may be procured from Mr. Bishop's quarry on Halkin, in Flintshire.

Thomas Stuart Traill, M.D.

No. II.

Mirfield Low Mills, March 7, 1821.

Sir,—After having tried your Halkin buhr-stones, for a fair and sufficient time, we are now enabled to lay before you a candid and faithful report of their quality; and this we shall endeavour to do, with as much brevity as is consistent with the importance of the subject.

The perfection of grinding consists, in reducing grain to a requisite degree of fineness, with the least pressure; or, in other words, to make the best and the greatest quantity of flour, out of a given quantity of wheat, with the least pressure. But, the mere good quality of a stone cannot effect this; for we must now call in the aid of art. And here it is that the great art of a miller consists, the putting of work into stones, or the obliquity and disposition of the furrows, every thing else compared with this being only trifles. And, indeed, when we consider that an accurate knowledge of this is grounded upon the doctrine of central forces, which constitutes an important branch of the Newtonian philosophy, we need not wonder that so few understand the real principles of corn grinding. We have, however, reasons to believe that we have considerably improved it.

From these observations it appears, that though the quality of the stones may be equally good, the effects produced will be different, according as the work is scientifically put in or otherwise; but, if the work and velocity of the stones be the same, we can clearly ascertain the quality of them by the effects produced.

We will now apply these observations to the stones in question. After twice or thrice taking them up, we were afraid that they would not stand the crack well; but this fear was soon dispelled, as we now find that they wear exceedingly little, and that the crack stands as fine as a hair. We now proceeded to ascertain the quality of the bran compared with our French stones, and for this purpose, we sifted the meal from every pair of stones as it came from the mill-eye; the bran thus retained in the sieve, we placed by itself, and by this means we had an opportunity of comparing the whole together. This we have repeated no less than forty times, and the result has always been, obviously from the very first glance, that the bran produced from the Halkin buhrs was not only cleaner, but of a more uniform cut; and this has not been perceived by millers alone, but by every person that has accidentally come into the mill.

This we think is quite sufficient to prove the superiority of the Halkin buhrs; but, that every possible doubt might be removed, we had recourse to the following experiment:

We

We selected the best French stones in the mill, made by the late Mr. Gardiner, of Liverpool, who was very famous for his knowledge of French buhrs; and, that the experiment might be the more accurate, we did not grind a quarter of wheat on each pair of stones, as it is impossible to part it from the wheat that precedes and succeeds with that degree of nicety that is required, without running the stones empty and thereby injuring them very considerably. But we weighed 480 pounds of meal, ground by each pair of stones, from the same wheat, weighing 57 pounds the bushel. These two parcels, after remaining a week, were weighed again, to see if any accession or diminution of weight had taken place; but the weights were precisely the same as before. The two parcels of 480 pounds each were then dressed, and the result was as follows:

Flour from the Halkin buhrs	390 pounds.
Flour from the French buhrs	384
Difference in favour of the Halkin buhrs	6

Now, in this experiment, the velocity and work of the stones being the same, the quality of the buhrs may be as justly inferred from the effects, or quantity of flour produced, as any other cause in philosophy from its effects.

We remain, sir, &c. &c.

J. & W. Pilling.

LXXXII. *Description of the Petrifaction Ponds at Shirameen, (a Village near the Lake of Ourmia, in Persia,) which produce the transparent Stone known by the Name of Tabriz Marble*.*

This natural curiosity consists of certain extraordinary ponds, or plashes, whose indolent waters, by a slow and regular process, stagnate, concrete and petrify, and produce that beautiful transparent stone, commonly called Tabriz marble, which is so remarkable in most of the burial-places in Persia, and which forms a chief ornament in all the buildings of note throughout the country. These ponds, which are situated close to one another, are contained in a circumference of about half a mile, and their position is marked by confused heaps and mounds of the stone, which have accumulated as the excavations have increased. We had seen nothing in Persia yet which was more worthy of the attention of the naturalist than this, and I never so much regretted my ignorance of subjects of this nature, because I felt that it is of

* From Morier's Travels in Georgia, Persia, &c.

consequence

consequence they should be brought into notice by scientific observation. However, rather than omit all description of a spot which, perhaps, no Europeans but ourselves have had the opportunity of examining, and on which therefore we are bound (in justice to those opportunities) not to withhold the information which we obtained, I will venture to give the following notes of our visit, relying upon the candour and the science of my readers to fill up my imperfect outline:—On approaching the spot the ground has a hollow sound, with a particularly dreary and calcined appearance, and when upon it a strong mineral smell arises from the ponds. The process of petrifaction is to be traced from its first beginning to its termination. In one part the water is clear; in a second it appears thicker and stagnant; in a third quite black, and in its last stage is white, like a hoar frost. Indeed a petrified pond looks like frozen water, and before the operation is quite finished, a stone slightly thrown upon it breaks the outer coating, and causes the black water underneath to exude. Where the operation is complete a stone makes no impression, and a man may walk upon it without wetting his shoes. Wherever the petrifaction has been hewn into, the curious progress of the concretion is clearly seen, and shows itself like sheets of rough paper placed one over the other in accumulated layers. Such is the constant tendency of this water to become stone, that where it exudes from the ground in bubbles, the petrifaction assumes a globular shape, as if the bubbles of a spring, by a stroke of magic, had been arrested in their play, and metamorphosed into marble. The substance thus produced is brittle, transparent, and sometimes most richly streaked with green, red and copper-coloured veins. It admits of being cut into immense slabs, and takes a good polish. The present royal family of Persia, whose princes do not spend large sums in the construction of public buildings, have not carried away much of the stone; but some immense slabs which were cut by Nadir Shah, and now lie neglected amongst innumerable fragments, show the objects which he had in view. So much is this stone looked upon as an article of luxury, that none but the king, his sons, and persons privileged by special firman, are permitted to excavate; and such is the ascendency of pride over avarice, that the scheme of farming it to the highest bidder does not seem to have ever come within the calculations of its present possessors.

LXXXIII. *Pro-*

LXXXIII. *Process of preparing Saltpetre, and Mode of manufacturing Gunpowder, in Ceylon**.

The preparing of saltpetre, and the manufacture of gunpowder, are arts which the Singalese, for many years, have constantly practised. The process of preparing the salt in different parts of the country was very similar. When the salt occurred impregnating the surface of the rock, as in the cave near Memoora, the surface was chipped off with small strong axes, and the chippings by pounding were reduced to a state of powder. This powder, or the loose fine earth, which, in most of the caves, contained the saline impregnation, was well mixed with an equal quantity of wood-ash. The mixture was thrown on a filter formed of matting, and washed with cold water. The washings of the earth were collected in an earthen vessel, and evaporated at a boiling temperature, till concentrated to that degree that a drop let fall on a leaf became a soft solid. The concentrated solution was set aside, and when it had crystallized, the whole was put on a filter of mat. The mother-lye that passed through, still rich in saltpetre, was added to a fresh weak solution, to be evaporated again; and the crystals, after having been examined, and freed from any other crystals of a different form, were either immediately dried, or, if not sufficiently pure, redissolved and crystallized afresh. The operations just described, were generally carried on at the nitre caves. In the province of the Seven Korles, besides extracting the salt at the caves, the workmen brought a quantity of the earth to their houses, where keeping it under a shed protected from the wind and rain, without any addition excepting a little wood-ash, they obtain from it every third year a fresh quantity of salt.

In their mode of manufacturing gunpowder, which is very generally understood, there is not the least refinement. To proportion the constituent parts, scales are used, but not weights. The proportions commonly employed are five parts of saltpetre, and one of each of the other ingredients of sulphur and charcoal. The charcoal preferred is made of the wood of the parwatta tree. The ingredients moistened with very weak lime-water, and a little of the acrid juice of the wild yam, are ground together between two flat stones, or pounded in a rice mortar. After the grinding or pounding is completed, the most seminated is collected, and carried in baskets to an adjoining stream, where it is well washed; the lighter particles are got rid of by a rotary motion given to the basket in the operation; and the residue, still wet, is transferred to shallow baskets for careful examination.

* From Dr. Davy's Ceylon.

LXXXIV. *On Embanking* 166 *Acres of Marsh Land from the Sea. By* Edward Dawson, *Esq. of Aldcliffe Hall, near Lancaster* *.

Aldcliffe Hall, near Lancaster, Nov. 10, 1820.

Sir, — I beg leave to present a claim to the Society for the Encouragement of Arts, &c., for the premium offered in No. 34 of their List of rewards published this year. I transmit the certificates required by the Society, and hope they will be deemed satisfactory.

The inclosure, the consideration of which I have the honour to submit to the Committee, consists of 166 acres, three roods, eight perches of land, known by the name of Aldcliffe Marsh, about two miles distant from the mouth of the river Lune, and one mile from Lancaster. It was, with the exception of about three acres, swarded over, and has heretofore been attached as a sheep pasture to the different farms on the manor of Aldcliffe; it was estimated at a low rent, as it was in a great measure overflowed by the spring tides, and being intersected by a deep pool, the sheep were frequently surrounded by the water, and consequently lost.

My first operation was, to convey the land waters from this pool into the Lune, which was done by opening for them a new channel through part of the old inclosures, from nine to twelve feet deep, and 246 yards in length. This cut was walled and covered with stone, and terminates with a hewn culvert of the same material, four yards in length, and two feet square.

On the 8th of May last, the embankment was commenced. It runs parallel with the Lune, which is in that part about a mile and a half in breadth at high water. The highest tides are with a south-west wind, which causes them to set in with considerable violence. The length of the embankment is 2010 yards; for the first 200 yards at the north (or higher) end, I satisfied myself with a slope of five horizontal to one perpendicular; in the next 1,400 yards, the slope is 6 to 1, and where the pool formerly discharged itself, it is for 300 yards 7 to 1; the remainder being on high ground, is 5 to 1; its height averages about 8 feet 6 inches; the greatest perpendicular height being 14 feet 6 inches; the whole of the inside slope is 2 to 1. It is entirely composed of sand, with the exception of the deep part, which is formed of clay, the sand being there worn away by the violent reflux of the tide. Its contents are as follows:—69,456 cubic yards of sand, covered by 53,078 superficial yards of sods

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for the year 1821. The Society's large Gold Medal was voted to Mr. Dawson for this communication.

or

or turf four inches thick, employing 3,824 horses, and 5,843 men, supposing it had been completed in one day.

In order to give employment to the poor of this neighbourhood, I contracted with five different persons; the whole was completed in August, many difficulties retarding it, from the unusual quantity of rain during the summer months. On the 29th of May, a violent storm of wind raised the tide, and swept away 1800 yards of material, which would have totally discouraged the contractors, who had no property, and could not have sustained the loss, had I not reimbursed them. I am thankful to say, the high tides in September and October have not made the slightest impression, and the whole of the work carries with it every appearance of stability. I apologize, sir, for the length of this communication; the desire expressed in the rules of the Society, that a detailed account should be given of works of this kind, must plead my excuse. I am, sir, &c. &c.

EDWARD DAWSON.

The equinoctial tides in September were the highest in the last twenty-four years.

CERTIFICATES.

November 10, 1820.

This is to certify, that Edward Dawson, of Aldcliffe Hall, has, during the summer of the present year, effectually inclosed and secured from the overflow of the tide, all that tract of land, near Lancaster, called Aldcliffe Marsh.

R. ATKINSON,
One of His Majesty's Justices of the Peace for the County Palatine of Lancaster.

November 10, 1820.

I do hereby certify, that Edward Dawson, of Aldcliffe Hall, has, during the summer of the present year, inclosed and effectually secured from the overflow of the sea, all that tract of land, near Lancaster, known by the name of Aldcliffe Marsh.

THOMAS BOWES,
Deputy Lieutenant for the County of Lancaster.

LXXXV. *On the Smelting of Tin Ores in Cornwall and Devonshire.* *By* JOHN TAYLOR, *Esq. Treasurer of the Geological Society**.

As I am not aware that the treatment of tin ores, or the mode of smelting them, has been recently described, and as the practice is confined to a certain district, it may be acceptable to the Society to have some account of the processes now used in Cornwall and Devon.

* From the Transactions of the Geological Society.

Tin ores are found in two kinds of deposits; first in veins accompanied by various other minerals; and, secondly, in alluvial matter in detached fragments.

It is usual in Cornwall not to apply the word ore to the oxide of tin, but to distinguish it, when in that state, by the term black tin, in contradistinction to white tin, which appellation is applied to it when smelted and in the metallic state.

The two kinds of tin ore above mentioned are, therefore, generally known by the names of mine tin and stream tin; and as they are for the most part smelted separately, and by different means, and as the metal produced from them is different as to its purity, it may be essential to point out the causes from which this diversity seems to arise.

Mine tin is, as I have mentioned, the produce of veins, and is raised with a mixture of all the substances which unusually accompany it. There are, not unfrequently, copper ores, pyrites, wolfram, micaceous iron, &c. and the separation of these, as also of the earthy matrix, is the object of various processes of dressing, which are conducted with the greatest care, and require a considerable portion of labour.

Whether, in a country where fuel for smelting is on the whole very cheap, it might not be œconomical to diminish the labour of dressing, and, by leaving more to be done in the furnace, reduce the expense of the former operations, is a question that I have never submitted to a direct experiment, though I conceive it to be one worthy of trial. The various earths may be quickly separated by fusion, as in the case of copper ores, which are now always smelted with a large mixture of the different kinds of spar in which they are found, all of which is easily run off by the fire, and the scoria or slag separated from the metallic part.

The fusibility of tin offers a mode by which it may be separated from an alloy of most other metals with which it is found to exist in veins, as lead and zinc ores are seldom mixed with it. This property is now made use of to a certain extent in refining tin, and might probably be taken advantage of still further, so as to avoid some of the charges incurred in dressing the ore.

The metal produced from mine tin is always of inferior quality, owing to the mixture of other metals, and which it is probable could not by any mode be entirely got rid of; it is known in commerce by the name of common or block tin, and the quantity forms a large proportion of the whole that is brought to market.

Stream tin is found in the lowest stratum of alluvial matter, in the bottoms of deep valleys, or places where a considerable deposit of mud, sand, and gravel, has been made by the action of water; it is often discovered occupying a thin bed incumbent on the

the rock, and covered by an *overburden*, as the streamers call it, which is sometimes from 20 to 70 feet thick. The tin is in rounded fragments, sometimes as large as walnuts, but more generally in the state of small gravel, and even of fine sand; it is imbedded in loose matter, composed of the detritus of the rocks from which it may be supposed to have been separated.

The principal peculiarity of stream tin is the absence of any other metallic mixtures, except nodules of hematitic iron ore, which sometimes accompany it. This circumstance fits it for producing a very pure metal. This is not the place to speculate on the causes which have so completely freed these ores from substances with which they were in all probability originally combined, or to inquire whether it is to be attributed to mechanical action, or whether it has been effected by decomposition; but it may be remarked that, besides the hematite already mentioned, only the indestructible metals, and the oxide of tin, are now discovered existing in deposits of this nature.

The operations of dressing stream tin are simpler than those for mine tin. It is smelted also in a different manner, and produces a superior metal known by the name of grain tin, which is principally used by the dyers, and for the finer purposes.

The processes for dressing mine tin are in many respects the same as are used for all other ores, but are subject to some variations, which are attributable to the following peculiarities.

1. Being for the most part found intimately dispersed throughout the matrix, the whole is necessarily pounded down to a very fine state, to admit of the perfect separation of the ores.

2. That being unalterable by moderate degrees of heat, it admits of calcination, by which the specific gravity of the sulphurets or arseniats with which it is mixed, may be lessened, and a mode obtained of rendering them more separable.

3. That the weight of tin ore being greater than most others, it is less liable to waste in the processes of washing, and, therefore, may be dressed so as to be nearly clean from all substances not actually adhering to it.

From the first of these peculiarities it follows, that all tin mines must be furnished with stamping-mills of sufficient power to bruise down the ores raised, which is generally done so as to produce a minute division of the whole, and on this account, formerly, the quantity and fall of water that could be applied to this purpose usually limited the quantity of ore that could be returned from a mine, or the whole was frequently carried to some spot favourable to the erection of water-wheels to be applied to this purpose. Within a few years steam-power has been applied to stamping-mills, and has tended to increase the supply of tin ores. Engines for this purpose, of considerable power, are

working with great effect at two of the largest tin mines in Cornwall, Wheal Vor and Great Huas; from which are now arising abundant returns of the metal, and where formerly it would have been impossible to have produced it.

The state of division, or the size, as the tin dressers call it, is regulated by a plate of iron pierced with small holes, through which the whole passes from the stamping-mill, being washed through by a rapid stream of water conducted upon it for the purpose. This is a point of great importance, and is regulated by the state of dissemination in which every ore is found.

It is not the intention of this memoir to detail the processes of dressing which are common to most ores, and, therefore, it may be sufficient to remark that, after being stamped, the tin ores are washed according to the usual mode, so as to separate the earthy mixture and as much of that of a metallic nature as is possible. All these operations are conducted with more than common care and accuracy; for, as tin ore holds such a large proportion of valuable metal, it is of course treated with every precaution to guard against waste.

Some metallic substances will be found, however, which, from their specific gravity approaching nearly to that of tin ore, or rather exceeding it, cannot be removed by any process of washing; these are mostly decomposable by a red heat, which the oxide of tin will bear without alteration. Therefore, after as much has been done as possible to render the ores clean on the dressing-floors, they are taken to the *burning-house*, which is furnished with small reverberatory furnaces, on the floor of which the ores are spread, and submitted to the action of a moderate and regular fire: they are frequently turned over by an iron rake to expose fresh surfaces, and a considerable volatilization of sulphur and arsenic takes place; the former seems principally to be consumed, and the latter is condensed by long horizontal flues constructed for this purpose. After the ores come from the burning-house, the process of dressing is completed by further washing, which is rendered easy by the alteration which has been produced in the relative weight of the substances.

Copper ore is not unfrequently present in these cases, and, as it is in part converted into sulphate of copper, the water which is first used is preserved, and a portion of copper obtained from it by means of iron.

The great specific gravity of the tin ore, as I have before remarked, renders it possible with care to subject it to many operations in dressing without much waste; and they are, therefore, applied until the whole is generally so clean, as to yield a produce of metal equal to from 50 to 75 per cent. In this state they are sold by the miner to the smelter, who determines their

value

value by assaying a sample, carefully taken from the whole quantity.

The furnaces for smelting mine tin are all of the common reverberating kind, and are of sufficient size to hold twelve to sixteen hundred weight of ore.

The charge is prepared by mixing it with a proportion of stone coal, or Welch culm, to which is added a moderate quantity of slaked lime; these are turned over together and moistened with water, which prevents the too rapid action of the heated furnace, and which would otherwise volatilize some of the metal before fusion commenced.

The heat employed is a very strong one, and such as to bring the whole into perfect fusion; it is continued seven or eight hours, when the charge is ready to draw. For this purpose, the furnace is furnished with a tap-hole leading from the lowest part of the bottom, which, during the process, is stopped with clay or mortar, and under which is placed an iron kettle to receive the metal. The furnace has also a door at the end opposite the fire-place, through which the slag or scoria may be raked out from the surface, while the tin is flowing out, by unstopping the tap-hole.

They are thus divided, and the tin is laded into moulds, so as to form plates of a moderate size, and put by for a further refining. The slag, which rapidly hardens into a mass, is removed to a dressing-floor, where, being broken up and stamped, it is washed, and a quantity of tin taken from it, which is called *Prillion*, and which is afterwards smelted again.

No operation in smelting is more easy than that practised for tin ores, nor is there any one in which the reasons for the mode of treatment are so obvious. There are but two things to accomplish in this first process; to obtain perfect fusion of the earths so as to suffer the metal to separate easily from them, and to decompose the oxide of which the ore uniformly consists.

The addition of lime contributes to effect the former, and that of carbonaceous matter or coal completes the reduction of the ore. The separation of the metal from the earths then takes place in the usual way during fusion, by the difference in their specific gravities, the one precipitating to the bottom of the furnace, from whence it is drawn off by the tap-hole, and the other, floating on the surface, is removed in the manner I have described.

The plates of tin, which are the produce of this smelting, are somewhat impure, and are more or less so according to the quality of the ore which has been used; they are reserved until a sufficient quantity of them is obtained to proceed with the refining, which is performed either in the same furnace, after ore-smelting

smelting is finished, or in a similar one, which may be reserved for the purpose.

All the processes for refining metals in the fire must be performed by taking advantage of some property in which the metal operated on may differ from those with which it is alloyed, and which it is intended to separate from it. These differences may consist in the facility or difficulty of oxidation, in their tendency to volatilize, in the temperature required for fusion, or in their relative specific gravities.

Upon an attention to the two latter circumstances is founded the operation for refining tin. The substances which are most to be suspected in the produce of the first melting, and which it is desirable to separate, will probably be iron, copper, arsenic, tungsten from the wolfram, which the miners call mock-lead, and a portion of undecomposed oxides, sulphurets, or arseniates, and of some earthy matter or slag.

The furnace for refining is raised but to a very moderate degree of heat, and the plates of tin being placed in it are suffered to melt very gradually, and the metal flows from the furnace at once into the kettle, which is now kept hot by a small fire placed beneath it. The more infusible substances will now be left in the furnace, and a further purification of the tin is obtained by agitating it in the kettle for some time by an operation which they call *tossing:* this is performed by a man with a ladle, who continues for some time to take up some of the melted metal, and pour it back into the kettle from such a height as to stir up the whole mass and put every part into motion.

When this is discontinued, the surface is carefully skimmed, and the impurities thrown up are removed; these consist of such matters as are lighter than the tin, but which are suspended in it, and, being disengaged by the motion, find their way to the top. In general, the metal is at once laded into the moulds, after the tossing and skimming are completed; but the produce of impure and irony ores may yet require that the tin be divided as much as possible from the mixture which may yet remain. This may be effected in a great degree by keeping the mass in the kettle in a melted state, by which the parts which are heavier than the tin will sink to the bottom, and by leaving a proper portion behind, the tin will be materially improved.

The last operation is that of pouring the metal into moulds, which are usually formed of granite, and which are of such a size as to make it into pieces of somewhat more than three hundred weight each. These are called blocks, and are sent, according to the provisions of the Stannary laws, to be coined by the Duchy Officers; and it then comes to market under the name of Block Tin, or a certain part which has been treated with more than common care is called Refined Tin. The

The making of grain tin from the ores from stream works is conducted in a manner altogether different, and remains to be described.

I have pointed out the purity of these ores, as regards their freedom from a mixture of other metals, and I do not think it important here to describe the mode of separating them by washing from the sand and gravel in which they are found, because the processes are very similar to those in use for dressing other ores. The stream tin is generally made very clean, and is carried in this state, to be sold for smelting, to establishments which are called blowing-houses, being thus distinguished from smelting-houses in which mine tin is reduced, and the term is also descriptive of the process employed.

The reduction of the ores for grain tin is performed by blast furnaces, and the only fuel used is charcoal. This mode of smelting is exceedingly simple, and is probably the most ancient one, as would appear from relics sometimes met with of furnaces of rude construction, and in some of which the wind alone seems to have been depended on for urging the fire.

The furnaces now in use are similar to those met with for smelting iron in foundries where the blast is used, and are formed by a cylinder of iron standing upon one end and lined with clay or loam. The upper end is open for receiving the fuel and ore, which are thrown alternately, and a hole at some distance from the bottom, at the back of the cylinder, is provided to admit the blast, and another, lower down and opposite to it, suffers the metal to flow out regularly as it is reduced.

A strong blast is kept up by bellows, or, in more improved works, by pistons working in cylinders, and the air is conducted by a proper pipe so as to blow into the orifice in the furnace.

The only purification it seems to require is to separate from it such substances as are mechanically suspended in it, and for this purpose it is laded into an iron pan or kettle, where the fusion is kept up by a gentle fire underneath, and a complete agitation of the mass is effected by plunging into the melted metal pieces of charcoal, which have been soaked in water, and, by means of an iron tool, keeping them at the bottom of the kettle. The water in the charcoal is rapidly converted into vapour, which rushing through the metal, gives it the appearance of rapid ebullition. After this is over, and the whole has rested some little time, the scum, which is thrown up to the surface, is taken off, and the tin, which is peculiarly brilliant in appearance, is removed by ladles into proper moulds, to form the blocks in which it is generally sold.

Grain tin is, however, sometimes put into a different form by breaking it: for this purpose, the blocks are heated to such a degree

degree as is known to render the metal brittle; they are then raised a considerable height from the ground, and, being suffered to fall, the whole divides into fragments, which assume a very peculiar appearance.

The smelting by a strong blast is injurious to metals that are volatilizable by heat, as they have in this mode no protection from the slag, which in reverberating furnaces floats on their surface, and protects them from oxidation and evaporation. The old practice of melting lead in what are called ore earths, is, on this account, giving way, and reverberating furnaces are coming into general use, by which the produce of metal from the ore is considerably increased. Tin, though volatile to a certain degree, is not affected by the process in any important manner; but, as some flies off in white fumes, it is usual to construct a long horizontal flue, which is made to communicate with and pass through a kind of chamber, in which a considerable part of these fumes is condensed and collected.

LXXXVI. *Successful Result of an Experiment on Draining of Land. By* John Christian Curwen, *Esq. M.P.**

London, Jan. 28, 1821.

Dear Sir, — Inclosed I have the honour to transmit for the Society a paper on Draining; if it should be considered as worthy of the attention of the Society, I shall be greatly flattered.

I have left the country in great distress, and numbers of poor people out of employment. I hope to have the honour of paying my respects to you soon. I disposed of the rice you sent me into various hands. I have planted the wheat in my own garden. I am, sir, &c. &c.

A. Aikin, Esq. J. C. Curwen.
Secretary, &c. &c.

Workington Hall, Jan. 17, 1821.

The encouragement given by the Society of Arts, for the improvement of agriculture, and every useful undertaking, emboldens me to submit to them the details of a work recently executed.

In the present state of the country, more important service cannot be rendered it, than suggestions for the profitable application of capital to labour.

Draining has universally been allowed to be the first and most essential step towards the permanent improvement of land. Fully as all writers are agreed upon this point, the cost that may

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1821. The Thanks of the Society were voted to Mr. Curwen for this communication.

profitably

profitably be expended in accomplishing this desirable object, is by no means ascertained; nor till a few months ago, should I have ventured to have estimated its advantages, as I feel myself now justified in doing. A recent occurrence brought this point strongly under my observation.

It may appear strange, that after twenty years assiduous attention to agriculture, I should not have formed a pretty correct estimate of the injury sustained from the want of a proper drainage of spring and surface water on any one crop; but so in truth was the case.

A field of 40 acres on the Schoose farm was last year cropped with Swedish turnips; the land was winter fallowed, and in the highest state of tillage, so as to admit of the turnips being sown in the latter end of April, previous to the long-continued wet, which proved so destructive to the turnip crop in the North of England: it had 30 tons of good dung per acre. The crop averaged on 38 acres, 32 tons and a quarter per acre, that is, twenty-six of bulbs, and six and a quarter of tops; the produce of two other acres scarcely reached twenty tons. The soil and management were the same throughout. It is a strong clay, by no means applicable to the growth of turnips; but the farm afforded no other soil more proper for the purpose. These two acres had by some means been overlooked when the rest of the field had been drained. The injury arose partly from springs, and partly from the surface-wet resting upon the land. The value of Swedes in common years is 10*s.* a ton for the bulbs; in the present year they would have sold at 15*s.* The loss, therefore, on 12 ton of bulbs, was eighteen pounds, besides the tops, which at 2*s.* 6*d.* a ton, would have amounted to 1*l.* 10*s.*, making a total of 19*l.* 10*s.*

Seventy-two roods of drains (seven yards to the rood) were immediately cut, the cost of which was 5*s.* a rood, or 18*l.*

Had the drainage been executed previous to putting in the crop, it would have been more than paid for by the produce of the present year.

That good often results out of evil, was never more fully exemplified; and with such a striking instance before me of the advantages resulting from completely freeing the land from water, I was powerfully stimulated to undertake the re-drainage of a field of eighty acres, adjoining the Schoose Farm-buildings, and within less than half a mile of the town of Workington.

I was still further excited by the daily and hourly applications for labour, arising, I fear, from the decreased and decreasing capital of the farmer.

The scale of labour has annually been declining, which cannot but be a matter of deep regret to every friend to the country.

The

The nation has witnessed scenes of great distress during the years of scarcity; but these bore no comparison to the present times.

The hope of the privations being temporary, gave courage to bear up against them: but now the future has nothing to invigorate exertion, or inspire fortitude. Numbers are daily forced into the ranks of pauperism against their will. Industrious habits are destroyed, and with them that providence and forethought which is the basis of the happiness and respectability of the working classes. In order not only to continue in employment the usual hands, but to extend it to the employing of others, at a season when the active labours of the year are nearly closed, I determined on undertaking the re-drainage of Walriggs, a field of eighty acres, which had been drained about 18 years before, in a manner then considered to be effectual.

The main-drains, as far as they go, were well done, and these have been made available in many instances in the present drainage. They all run into the ditches which surround the whole, from which there is a considerable fall on every side of the field. The collateral drains were only twenty inches deep, set with three stones, in the form of a triangle, having about eight inches of cover upon the top. A drain of 20 inches was then thought to be sufficient, and all that was aimed at, was to cut off the springs, no regard being paid to carry off the rain-water, which is so injurious to clay land.

Subsequent experience has shown that, in most instances, the stratum which holds the water is at so great a depth, as to be below the bottom of such shallow drains; that to do the work effectually, the drain must reach the stratum where the wet rests.

The importance of deep ploughing was not heretofore known, or provided for.

Five years ago this field was deep ploughed; it had been foreseen, that in many instances the plough was likely to come in contact with the head of the drains: this did happen, and the consequence has been to render the land as wet, or nearly so, as it was before any thing was done to it.

Fifty out of the eighty acres were greatly injured by water. The annexed plan will point out the manner in which the work has been executed. It was commenced in November, and was finished the second week in January.

The cutting was let, as it requires practice to keep the drain the exact width. Bad hands are apt to increase the dimensions, and thereby greatly augment the expense of filling, which is the expensive part of draining. Gathering and getting stones was done by the day, and employed a number of women and children, besides the persons occupied in the quarries, which were fortunately near at hand. The depth of the drains is from 3½ feet

to

to four feet; the breadth, twenty inches at the top and twelve inches at the bottom. The drains have a cavity at the bottom of six inches, being set with two side stones, and a cover, and then filled with stones to the top, the six inches next the top being filled with small stones, that in case the plough should strike into them, no injury is done to the drain. The drains are thus filled to within ten inches of the surface. It required a solid yard of stone to fill a rood of seven yards; in weight above two tons.

To furnish such an enormous quantity of stones as eight hundred and fifty-nine roods required, was an undertaking of no small difficulty, and could not have been executed in the time, had not other substitutes been found. In coal countries there are strata known by the name of sill or schistus, and rattler, which is a mixture of coal and schistus. Sill is a substance that will not bear exposure to the atmosphere, but rattler does not fall, and is very light in comparison to its bulk.

Recourse was had to these substances, and many hundred cart-loads of both were collected from the coal-banks, the remainder was gathered from the ground, and obtained from the quarries.

	s.	*d.*	
The cutting, filling, and setting was	1	3	a rood
Collecting stones, supposing two gathered to each rood .. .	0	8	
Two carts from the quarries .	1	0	
Leading	2	0	
Cutting the drains by the plough	0	1	
	5	0	

The distance the sill and rattler had to be led, so increased the cost of cartage, as to make their cost equal to that of stones.

	£	*s.*	*d.*
Cutting and filling 859 roods of 7 yards, at 1*s.* 3*d.*	53	13	9
3,436 cart-loads of stones for filling, at 10*d.* a cart	143	3	4
Carting the above, at 6*d.*	85	18	0
Filling, at 1*d.*	14	10	6
	297	5	7

Fifty acres of the field have been benefited by this drainage. The general quality of land deciding the value at which it would be estimated to let, it was considered as worth 40 shillings an acre; from its locality, I conceive I am within bounds, when I rate it as worth from 50 to 55 shillings. The expenditure of two hundred and ninety-seven pounds, has added sixty pounds to the value of the field, which is obtained at five years purchase, or a little less for interest. It is to be observed, the horse-work is valued as if it had been hired; the real cost of that part, done at such a season, is not, to a farmer, one-half. My object was

 to

to put the cost at the highest point, more strongly to enforce the advantage resulting from the practice, as it thus leaves nothing to object to.

This field had in the last course 30 tons of manure; it is strong clay. First crop, potatoes, product 26 hundred stone per acre: sown with wheat and clover; both these crops were admirable. The oats this last year are calculated to produce 60 Winchester bushels per acre; it is now preparing for green crop again, and to have 50 tons of manure per acre. Admitting the green crop to profit three pounds per acre by the drainage, which is only half what was lost at average prices this year on the Swede crop, this on the 50 acres would be one hundred and fifty pounds: calculating it to yield three Winchester bushels per acre more of wheat, at 7*s.* per bushel, this would be fifty-two pounds ten shillings and ten-pence per acre; for the clover for two years 50*l.* more, making a probable increase of produce, without any extra expense, of 252*l.* 10*s.* Thus, in a five years course the whole expense will, in all probability, be repaid, and an annual permanent increase of rent to the amount of 60 per cent. gained.

Wet is more destructive to pasture than it is to grain and green crops; and as pasture is the most material object near to towns, draining, in such situations, is a more profitable improvement than in any other situation, and will consequently justify a greater expense.

When once dry land is well laid down to pasture, the improvement is permanent. If flooded with water, it cannot remain for any length of time in pasture, but must be again brought under tillage. On wet soils, improvement is almost labour in vain—costly at all times, but now ruinous.

Should the Society deem this undertaking as meriting their attention, it will be highly gratifying to me, who owe them many and great obligations.

The ambition of meriting the honour of their rewards, first directed my attention to agriculture, and I trust the result has not altogether been without its advantages to the public.

I am, sir, &c. &c.

JOHN CHRISTIAN CURWEN.

LXXXVII. *Account of a Volcanic Eruption in Iceland. By Dr.* FORCHHAMMER*.

THE very low state of the barometer throughout a great part of Europe in the months of December and January, although not

* From Annals of Philosophy, No. 18.

immediately

immediately followed by any eruption of the volcanoes in Italy, excited apprehensions of violent volcanic phænomena in Iceland; and in the month of March, letters were received in Copenhagen from which the following account is drawn up.

In the beginning of the month of September, the frost began on the east coast, and on the east part of the north coast of Iceland, with a violence that was quite unexpected after the experience of the preceding years. An amazing quantity of snow fell, and the Greenland ice surrounded the whole east and north coast, accompanied as usual by continual snow and frost. It was remarkable that the fine weather continued on the south coast of the island till the beginning of November, the lowest state of the thermometer at Næss, near Reikiavig, being on the 23d and 24th of September $=41°$ Fahr. On the 19th of October it suddenly fell to $23°$ Fahr., which lasted, however, only for one day, and before and after that time the temperature of the atmosphere was constantly above the freezing point, until on the first of November, when constant frost began.

The island, though frequently alarmed by earthquakes, had experienced no volcanic eruption since that famous one of 1783 and 1784 from the Skaptaa-Jokkul, which destroyed such a great part of the cultivated lands, except some small eruptions which were said to have taken place in the interior, far from the inhabited part of the island, and which passed away without attracting further notice, when in December 1821, a new crater was suddenly formed on the Eyafjeld-Jokkul, a mountain of which, among the numerous volcanic eruptions, only a single one is mentioned, in the year 1612, when a great part of the ice of the mountain burst, and was thrown into the sea.

The Eyafjeld-Jokkul (known among sailors under the name of Cape Hekla) is the highest of all the mountains in Iceland; and, according to the last measurements, is 5666 feet high. It is the southernmost of the chain of mountains in which the dreadful eruptions in the middle of the last century took place, and at about equal distances from the Kolla and Hekla. From 1024 to 1766, twenty-four eruptions are recorded to have occurred. That part of the mountain where the crater was formed borders two sides the cultivated land, which belongs to the hundred (Syssel) of Rangarvalla, in the south part of the island.

The following account is an extract of a letter from M. Bryniulo Sivertsen, Minister at Holt, in Eiafields-boigden, to the Bishop of Iceland, M. Vidalin:—"The real crater is about five miles from my house at Holt. The fire made its way suddenly by throwing off the thick mass of ice which scarcely ever melts, and of which, one mass, 18 feet high, and 20 fathoms in circumference,

ference, fell towards the north, and, therefore, fortunately not over the village. At the same time, a number of stones of different sizes slipped down the mountain, accompanied by a noise like thunder; no real earthquake, however, was felt. After this, a prodigiously high column of flame rose from the crater, which illumined the whole country round so completely, that the people in the house at Holt could see as perfectly at night as in the day time. At the same time much ashes, stones, gravel, and large half-melted pieces of the rock, were thrown about, some of which amounted to the weight of 50 pounds. In the following days, and until the new year commenced, a great quantity of fine powder of pumice fell in the surrounding country according to the direction of the wind, so that a thick bed of it covered the fields. It resembled the falling of snow, and penetrated through all openings into the houses, where it exhaled an unpleasant smell of sulphur. The eyes suffered extremely by this dust. At Christmas, a violent storm from the south raged; it rained hard, which produced the good effect of blowing and washing away the ashes from the fields, so that they will do but little harm. We think ourselves extremely fortunate that so frightful a revolution in our immediate neighbourhood has produced no bad effects either on men or animals."

Another extract of a letter from M. Terve Johansen, the Provost at Breidebolstad, about 18½ miles to the west of the volcanoes, dated the first of February 1822, gives the following additions: "We still see the column of fire of the volcano shining with the same clearness as in the beginning, without, however, throwing lava into the inhabited part of the island. The ashes are greyish-white, have a sulphurous taste, and it is reported that they burn with flame when thrown into the fire. The ice of the Jokkul was twice broken, and an eye-witness has assured me that some of the pieces were three times as high as himself, and of many fathoms in circumference. Among the numerous half-melted stones, one has been found thrown to the distance of about five miles from the crater. We have had no accounts of the bad effects of this eruption either on men or animals. The thick mass of ashes spread over the land of Vester Eyafield and Oster Landoe, which began to occasion diseases among the sheep, has been blown away by a heavy storm, and since that time the wind has carried the ashes from the volcano into the uninhabited mountains; the diseases among the sheep soon disappeared."

The third account is from M. Steingrim Johnson, Provost at Rangarvalla and Vestmamoesyssel, and written from Odde, about 30 to 35 miles to the W. of the volcano, dated December 19, 1821.

"On

"On Wednesday, December 19, at twilight, and later in the evening, a reddish light appeared on the E., which was the more surprising, as it was clear.

Dec. 20.—At one o'clock in the afternoon, a number of rather shining clouds was seen collected about the top of the mountain above Eyafjeld-Jokkul, E.S.E. from Odde; the clouds soon changed into a high column of smoke increasing in thickness and darkness. Though the weather was clear and calm, the smoke was carried to the south; at sunset, the eruption seemed to cease, but the smoke soon rose again, and even more violently than before. When it was dark, we clearly saw the moving and the sparkling flame; from which we concluded that the eruption must be violent. Afterwards we heard that it was on the east or south side of the Vesterjokkul, near Hudnasten, and opposite to the farm-house of Skaale, in the parish of Holt.

Dec. 21.—There was a violent storm, and the fire was observed varying in intensity; clouds of smoke rose with great violence. They remained on the mountain, and to the west of the Jokkul, whose white brilliant colour was now destroyed by the shower of ashes.

Dec. 22.—The same phænomena; the clouds increased, and spread all over the sky, principally towards the south.

Dec. 23.—The same smoke. In Hvols-Reppen, and in this parish, the people believed that they saw the falling of ashes which came from the north-east. Afterwards we were told that a great quantity of them had fallen that night, and before, in the villages that were nearest to the volcano.

Dec. 24, 25.—The clouds of smoke remained on the same place, and in the same direction, as before; now and then the fire was observed on the place of the first eruption.

Dec. 26, 27.—Heavy storm from north-east; the clouds of smoke on the same place.

Dec. 28.—The weather began to get more calm; it seemed as if the column of clouds was divided into two, which took different directions by different currents of wind.

Dec. 29.—Weather calm and pleasant. The clouds of smoke moved towards the north and east over the ice mountains. Late in the evening a mild rain.

During this whole time, the cold was moderate, not exceeding 25° Fahrenheit, and sometimes it was 4° above the freezing point. It is reported that the water of the river which falls into the ————, and in the other rivers that come from the Jokkul and the surrounding mountains, had increased considerably during the first days of the eruption. In the vicinity of the volcano a constant rumbling noise was heard, now and then accompanied

panied by a dreadful crash, as if the whole immense masses of stone and ice were going to fall together. The greater part of the ashes was fortunately carried towards the north, into uninhabited mountains and heaths, where also a great quantity of pumice has fallen."

In another letter from the same Provost, dated Feb. 23, it is said, "The clouds of smoke have not yet disappeared, and today they are increased. No ashes, however, have been observed during a long time, and the Jokkul has resumed its shining white colour, so that the rain and wind must have removed the ashes. The smoke greatly resembles the steam rising from boiling water, and certainly owes its origin to the fire below. Some imagine they have observed that the Jokkul has decreased, and is now lower near the crater, which certainly must now be larger than before, the column of clouds increasing in circumference. So it appears at least from this side from N. to S.; but whether the same has taken place in the other direction, from E. to W., I am not able to say. It has been reported that to the E. two other volcanoes have had eruptions, the mountains Katla and Oraefa Jokkul, but nothing is known about it. Since the eruption, the weather has become worse, owing to its unparalleled variableness, storms, and afterwards cold, and a great quantity of snow."

Dr. Thorsteinson, in a letter to Prof. Oersted, gives the following additions:—"Since the first of January, the violence of the eruption has been decreasing. Though the town of Reikiavig is about 74 miles from the volcano, the flame was observed there several times at night, when the weather was clear. People that recollect the eruptions of 1766 and 1783 think this trifling, but principally because it has done no harm. Though distant about 74 miles from the volcano, I thought that the weather became much milder after the eruption. Though the barometer was pretty low during the eruption, yet it was lowest on Feb. 8, when it was only 27·25 inches; but the fire did not increase, nor did we feel any thing like an earthquake; but near the volcano, they had constantly small shocks."

The vessel which brought this news had left Iceland on the 7th of March, and it is reported that the sailors when at sea again saw a violent fire.

State of the Barometer and Thermometer from the Beginning of December to the End of February, at Næss, near Reikiavig, in Iceland. By Dr. THORSTEINSON. (Reduced to English Measures and Fahrenheit's Thermometer.)

1821.	Barom	Ther	1822.	Barom	Ther.	1822.	Barom	Ther.
Dec. 1	28·99	23½	Jan. 1	29·75	39	Feb. 1	29·34	12
2	28·64	23½	2	29·84	27½	2	29 35	14
3	28 75	12	3	29 90	27½	3	29·23	17
4	29·32	3	4	30·15	23½	4	29·07	23½
5	29·38	23½	5	30·18	34	5	29 05	17
6	29·43	27	6	30·18	34	6	27 99	17
7	29·46	20½	7	30·12	33	7	27·88	18½
8	29·49	18½	8	30 08	33	8	27 25	25½
9	29·55	18½	9	29·32	25	9	28·70	34
10	29 61	27	10	29·62	25	10	29·05	27
11	29·03	34	11	29·68	25	11	29 42	27
12	29·18	36	12	29·63	23½	12	29·32	21
13	29·12	39	13	29·49	25	13	29·16	34
14	29·18	27	14	29·48	23½	14	29·05	27
15	29·25	32	15	29·25	32	15	28·99	27
16	29·10	39	16	29·23	23½	16	29·57	25
17	29·12	41	17	29·60	25	17	28·56	27
18	29·16	41	18	29 52	23½	18	27·72	27
19	29·14	34	19	29·05	25	19	28·25	27
20	29·04	30	20	29·15	25	20	28·33	36
21	28·70	25	21	29·34	20	21	28·49	25
22	28·53	23½	22	29·78	23½	22	28·63	23½
23	28 57	25	23	29·84	25	23	28 45	18½
24	28·54	25	24	30·05	32	24	29 66	8
25	28·52	27	25	30·06	23½	25	29·68	9
26	28·49	30	26	30 02	30	26	29·60	11½
27	28·58	27	27	30·00	27	27	28 76	36
28	28·99	54	28	29·40	30	28	29·11	25
29	29 12	34	29	29·13	25			
30	29·13	36	30	28·96	25			
31	29·83	36	31	29·06	21			

LXXXVIII. *On a particular Construction of M.* AMPERE'S *Rotating Cylinder. By Mr.* JAMES MARSH, *of Woolwich. Communicated by* P. BARLOW, *Esq. Royal Military Academy.*

To Dr. Tilloch.

DEAR SIR,—THE inclosed communication from Mr. Marsh relates to one of the most pleasing experiments in Electro-magnetism. In its original form it is due to M. Ampere; but it is much

much improved by the construction explained in the letter. As it has not yet, I believe, been given in any English work, it will, I am sure, be interesting to many of your readers.

I remain, dear sir,

Yours very truly,

Royal Military Academy, June 11, 1822. PETER BARLOW.

May 31, 1822.

SIR, — HAVING been lately employed in constructing for Mr. Barlow one of M. Ampere's rotating cylinders, a new form of suspension suggested itself to my mind, which, upon trial, succeeded admirably; and as it seems to add much to the interesting nature of the experiment, I have been induced, by the advice of the above gentleman, to give you the following description of it, under the hope that you may be disposed to give it a place in your valuable publication. I remain, sir,

To Dr. Tilloch. Your obedient servant,

JAMES MARSH.

The instrument alluded to is represented in Plate V. fig. 1 being a perspective, and fig. 2 a section of it. A B C D is a cylinder of very thin copper, about one inch and a half high, and two inches in diameter; *a b c d* is another copper cylinder of less diameter, soldered to the bottom of the former at *dc*, where there is a circular hole to receive it; so that within the space A *a*, D *d*, B *b*, C *c*, a quantity of diluted nitric or sulphuric acid may be introduced; *e f g h* is a very light hoop or cylinder of rolled zinc. To the copper vessel *a b c d* is soldered a thin copper wire *a i b*, having a small socket at its upper part *i*, to receive the point proceeding from the other copper wire *e k f*, soldered at *e f* to the zinc cylinder. N S is a cylindrical magnet, which is represented as broken in the figure, but which (when the instrument is used) has its lower end inserted in a foot or stand; at its upper end is a small agate cap to receive the point proceeding downwards from *i*. If now (the magnet being first placed vertical) the cylinders be suspended, as shown in the figure, and the copper cell ABCD be nearly filled with diluted acid, the two cylinders will begin to revolve; the one from left to right, and the other from right to left; the rotations under favourable circumstances amounting to 120 in a minute with the zinc cylinder; but the motion of the copper cell, from its greater weight, is not so rapid. With the north end of the magnet upwards, the zinc cylinder revolves to the left, and the copper vessel to the right; and if the magnet be inverted, the motions of the two cylinders will be inverted also.

It is proper to observe, that M. Ampere's construction is the same

same as the above, with the exception of the lower descending point and agate; and consequently in his machine only one motion can be produced; whereas, by the second suspension, we exhibit at once the compound motion, and show the opposite effects of the connecting wire proceeding from the opposite sides of the galvanic apparatus. It will, of course, be understood that the magnet is of such diameter as to admit a perfect freedom of rotation about it.

LXXXIX. *Description of the Gooseberry Caterpillar; and practical Means for preventing its Ravages.*

To the Editor.

As the season has now arrived when that voracious little animal, called the gooseberry caterpillar, commits such universal devastation in our gardens, I have taken the liberty to send you a particular description of the fly from whence it proceeds, together with a remedy for preventing its ravages; and, if you think that so much said about so diminutive a creature is worthy of a place in your Magazine, it is at your service for publication.

The caterpillar is too well known to need any description, but it does not seem that the fly from which the caterpillar proceeds is: I am sure that it is not; and that many people imagine that it comes from a moth or butterfly, which I know it does not; and I am quite sure that the following account is correct. Nor has there been, that I have ever seen, any published account how its depredations may be prevented; and, from the observations which will be presently made, if the suggested remedy should not prove effectual, it may open the subject to the minds of those who may discover something that will.

In the first place, I will give the description from Sturt's "Natural History of Insects," 2. b. 166:

"93. *Phalæna wavaria*, Gooseberry M.—Wings cinereous*; the upper ones with four abbreviated unequal black *fasciæ*†. Inhabits Europe. B. The caterpillar feeds on the currant and gooseberry: it is somewhat hairy, green, and dotted with black; having a yellow line along the back, and two on the sides. About the middle of May it goes into the ground, to change into a naked brown-pointed *pupa*‡. About the middle of June the moth appears, which is very common."

Now the above description is extremely imperfect; as well as

* Cinereous—having the appearance of being covered with ashes.

† *Fascia*—a broad transverse line.

‡ *Pupa*—the *aurelia* ....

 materially

materially incorrect; at least for the southern and warm part of Devonshire, where the fly from which this destructive little animals proceeds first appears about the latter end of March, or the beginning and throughout the month of April, just as the gooseberry leaves have attained a sufficient size for them to deposit their eggs on, and to supply their young with food; which eggs are invariably placed on the inside rib of the leaf, and the flies always first select those leaves nearest the ground, which proceed from the rank water-shoots in the middle of the bush (this is very material to be known, as will hereafter appear); and, when these interior leaves are consumed, the caterpillars then gradually ascend, until the whole bush is denuded, and, consequently, the fruit spoiled.

To those who are unacquainted with the fly itself, a particular description of it may not be uninteresting. The flies, if attentively observed, may be first seen in the latter end of March and the beginning of April, as before remarked; but the first notice that we have of the destroying caterpillar is the skeleton leaves, and, when it has done most of its mischief, then people set about picking them off; but this, though it is a temporary relief, is a troublesome task, and an endless and ineffectual remedy; because, though many adult caterpillars are removed, there are thousands still left behind in the egg, on the inside of the leaves, which cannot be discovered without turning every leaf upside down: the eggs are then easily discovered, like as many little pearls, from a dozen to twenty in number, about the size of pin's heads, not round but oval, and whitish. It is seldom that the first stock of flies do much mischief; the leaves grow too rapidly for the caterpillars to destroy, and they are supplied with sufficient food until they drop into the ground; they are then formed into the *pupa*; from whence, after a short time, a second generation of flies are produced, who perform the same operations of increase and mischief as their parents, and so on to a third, a fourth, and fifth, when the season is favourable, until the approach of winter puts an end to their devastations. The last or autumnal caterpillars fall into the ground, where they remain in *aurelia* state until the succeeding spring. I have some now by me in a box, that I put aside in October last, which are not yet changed into the fly. In an unfavourable season, we seldom see any after the first appearance. Upon the season, then, and other causes, depend all the first and successive operations of this pernicious little reptile, the name of which it is necessary to know before any remedy can be applied.

Mr. Sturt seems to understand that the caterpillar first appears; the fact is, that the fly first appears; as is agreeable to the nature of all insects which undergo the common transformation

tion of the butterfly tribe. I will endeavour to give an exact description of the female fly. In the first place, it is a very dull, stupid, little animal, that will allow itself to be caught without the least difficulty: it has two horns or feelers; a head very dark, with two large eyes; four transparent wings; the body or carcase a light orange colour, when full of eggs not so large as a grain of wheat; the shoulders dark, to which are affixed six legs, three on a side, also orange colour, having three joints, five black spots on the last joint of each leg. It is a fly in every respect, having no resemblance whatever to a moth or butterfly; and, with the exception of the horns or feelers, and yellow body, it is very much like the small house-fly, the wings being quite smooth and transparent, resembling fine isinglass, of a snuff-colour tint, and free of all that down or feather which covers the wings of butterflies and moths. Still it must be admitted to be among the genus of the moth or butterfly; as they do not appear to take any food, and undergo the common transformation from the egg to the caterpillar, the *aurelia*, and the fly*. There is a black stripe on the outer part of the two largest wings. The whole insect is not above the third of an inch in length, which seems the more surprising, as it produces such a pernicious race of destructive caterpillars, at their full size nearly an inch long. Their habit is to perch on the outside of a gooseberry or currant leaf, and then immediately to creep on the inside, when they directly begin to drop their eggs on the ribs of the leaf. Thus, to a person who does not know the fly, and watch her motions, the the parent of these millions of insects is unknown; and people wonder, as the cause is unseen, from whence and from what these caterpillars proceed: but something cannot come out of nothing. It is generally imagined that they proceed from a moth or butterfly; yet it is admitted that no moth or butterfly is ever seen about these bushes; but the fact is, that the mother of all this mischief is the little fly which I have described.

The above description is that of the female fly I accidentally saw perch on a leaf. A gentleman who was with me, and myself, watched her operations, and she did not seem at all molested at our moving the leaf, to see what she was about: we noted the time, and in eight days the eggs then deposited were hatched into caterpillars. Thus, all the mischief is done in secret and quiet; and, whilst hundreds of these flies are in a garden, the cause is not known, and the injury is not seen, until it becomes irremediable. When first hatched, they gnaw only the inside of the leaf; but, as they get older and larger, they feed

* "A fly in entomology is an order of insects, the distinguishing character of which is, that their wings are transparent. By this they are distinguished from moths, butterflies, &c.'

upon

upon the edge of the leaf, until the whole is consumed, and then they retire by the stem to the next leaf; and so on, until every leaf is destroyed. In about a fortnight the caterpillars attain their full size, and then drop on the earth,—into which, or into the crevices of a wall, or other convenient place, they creep, where they are lost sight of, and are transformed into the *pupa*.

The male fly is so very unlike the female, that, if I had not seen them united, I should have taken it for a different species; and I never saw this union but once. The body of the male is rather longer and darker than the female, and not larger than a common pin, and is much more alert and active; still it partakes of the dulness of its mate, and will allow itself to be caught without any difficulty.

During the growth of the caterpillar, it is needless to notice its extreme voracity; the skeletons of the leaves are a sufficient proof of that fact*. The evil is the destruction of all the fruit, as a consequence of the destruction of the leaves. This is a disappointment to many, and worthy an inquiry of considerable magnitude; and this has induced me to be so particular in the description of an animal in other respects only entitled to common curiosity. But I know no insect, except the turnip flea, or fly, that is of so injurious a disposition as the gooseberry caterpillar, and therefore I have given its history and nature in detail, that, if possible, its ravages may be prevented.

Now as to the remedy: As the fly first makes its appearance in the latter of March and April, and afterwards, according to the season, or other causes which we are unacquainted with, appears throughout the summer, it strikes me that the only remedy is by placing something about the stem, or among the branches of the bush, the smell of which is obnoxious to the flies, and which they will not approach; and I have been assured, by a gentleman who had repeatedly made the experiment, that the smell of coal-tar would, as he called it, keep off the caterpillars; the fact is, that it kept off the fly. His practice was to wrap a beam or twist of reed, strongly impregnated with this strong-scented bitumen, round the stem of the bush; and no caterpillar touched a leaf. If there be no fly, there can be no caterpillar. There was not a leaf eaten upon this gentleman's bushes, when all his neighbours' were destroyed, and the fruit of course spoiled.

I have heard of other remedies,—such as, soap-sud water

* Thus have I seen the fly produced from the caterpillar in a box, the male and female united, and the female lay her eggs, which came to caterpillars; and I have now several aurelia. So that there can be doubt but that the caterpillar comes from the fly which has been described, and not from a moth or butterfly, as is generally supposed.

thrown

thrown over the bushes, lime, and chimney-soot, and a strong decoction of elder leaves; but who can eat gooseberries and currants after they have been besmeared with such filthy materials, which at best apply to the evil in part? But, if any one can discover a means of keeping off the fly by the smell of something which is disagreeable to it, it goes to the root of the evil at once; and there is nothing in the smell of coal-tar which can excite a prejudice in the most delicate stomach. If this should not generally succeed, what has been said upon the subject may perhaps be the means of some of your chemical and philosophical correspondents finding out something that will. Black pepper keeps off the flies from meat, and it is by no means impossible that a discovery may be made to keep these flies from the gooseberry-bushes: for I am well assured, that there can be no effectual remedy for this evil, but the discovery of something the effluvia of which will produce this effect: and the season is now approached when the attempt should not be neglected: for, if the first invasion succeeds in making a lodgement, it may not be so easy to prevent a second and a third from taking entire possession of all the bushes. It is upon this principle of creating an offensive smell, that turnip-seed is recommended to be steeped in train-oil before it is sown; and it is said to be a perfect security against the bite of the turnip-fly.

Totness. J. C.

XC. *On an Insect which is occasionally very injurious to Fruit-Trees. By* WILLIAM SPENCE, *Esq. F.L.S.**

My attention was first attracted to this insect some years ago, by observing small masses of saw-dust-like excrement, the usual indication of the presence of larvæ, protruding from the edges of the cankered parts of a very diseased summer *apple*-tree, of the name of which I am ignorant. On cutting off a portion of the wood, I found many small white larvæ inhabiting cavities which they had excavated between the bark and albumum, and sometimes wholly in the latter, upon which they seemed to feed. These larvæ were of different sizes, and amongst them were several chrysales, which having detached, and placed under a glass, they produced in a few days the *Tortrix Wæberana*, a small moth very abundant in the garden, and thus proved to be the parent of the larvæ.

I at first supposed that these insects, like many others, deposited their eggs only upon parts of the trees previously diseased. Even on this supposition, their injurious effects would be

* From the Transactions of the London Horticultural Society.

very

very considerable, as it was clear that they every year greatly enlarged the extent of the canker, not merely by devouring the neighbouring alburnum, but by forming numerous cells in it, which when quitted by the chrysales are filled with water by every shower, and thus become the source of more speedy and extensive decay. Many of the cankers in the tree above alluded to, have eaten half-way through the small trunk and branches, which if not sheltered by a wall must have been long ago broken off by the wind.

This tree is a remarkable example of the effect of partial decortication, as recommended by Dr. Darwin (*Phytologia*, p.378), in inducing the production of flower instead of leaf-buds. Not only the bark, but half the trunk, as above observed, is eaten through in many places; yet though a new twig is scarcely ever put forth, it never fails to be laden with blossom and fruit. Here I may observe that a similar result, as to the increased produce of fruit, and the paler green of the leaves, with that above referred to by Dr. Darwin, I have myself seen on a branch of a *pear*-tree, from which nearly a complete cylinder of bark had been gnawed by cattle. It was filled with fruit, while not a pear was to be seen on the rest of the tree.

Narrower examination, however, has shown me, that their attacks are by no means confined to the diseased parts of fruit-trees; nor directed, as I at first conjectured, against the *apple*-tree only. Being more anxious to ascertain the œconomy of an injurious insect, than desirous of preserving the tree which they chiefly attacked, I took no steps for extirpating them; and they have, in consequence, seemed to increase every year since I first observed them, and last year carried on their operations so extensively, as to threaten more serious injury in return for my forbearance, than I had calculated upon. Not only were they more than usually abundant near the margins of all the old cankers, but I observed their masses of excrement adhering, in every direction, to the surface of the healthiest *pear*- and *apple*-trees in the garden; and wherever these indications appeared, the application of the knife aways detected the caterpillar beneath.

It is thus evident that, where they abound, no other cause is wanting to generate canker and disease. Though their attacks upon the bark ond alburnum should not at first be extensively injurious, the admission of water into their empty cells, and frequent repetitions of the mischief, must, in the end, cause rottenness; and it is perhaps not improbable that to these insects should be often primarily attributed the canker laid to the charge of the soil, or the mode of cultivation.

After these prefatory remarks, I shall proceed to describe the insect in its different states, adding such observations as have occurred

occurred to me upon its œconomy, and the most probable means of extirpating it.

Eggs.—I have never been able to detect any of these upon the parts of the tree where I conjecture they are laid; but several were deposited on the sides of a glass jar, under which I had kept the two sexes from their first exclusion. They are lentiform, flat below, slightly convex above, smooth, pale red in the middle, with a white and apparently membranous margin. Altogether they very much resemble the seeds of the common garden *stock*, except that they are not above one-fourth of the size; and they presented an appearance so very dissimilar to that of the eggs of insects in general, that I for some time overlooked them.

Larva.—The eggs above mentioned not having produced any larvæ, I am unable to say any thing as to the precise period at which they are hatched; but from observations made on those found in the fruit-trees, I conjecture that they appear very shortly after the eggs are laid, and immediately proceed to insinuate themselves beneath the bark. When full grown, they are from four lines to half an inch long, and about a line broad; and wholly of a dull semi-transparent white colour, except the head, which is pale chesnut, which with the adjoining segment is also sometimes tinged. In some specimens, an obscure reddish line runs along the body, which is owing to the red colour of the fluid contained in it. The body, besides the head, consists of twelve segments, which, owing to the wrinkles in the three first, are not very easily counted. To each of the three first segments below, are affixed the usual pair of clawed feet, the claws of which are sometimes yellowish; and a pair of tubercular or false feet, as they are often called, are attached to the 6th, 7th, 8th, 9th, and 12th (or last) segments: so that in all the insect has, as is usual in this tribe, sixteen feet; six clawed, and ten tubercular. Each of the segments above, is furnished with from four to six slightly elevated protuberances or mamillæ, more polished than the rest of the body, of a rather darker colour, and having one and sometimes two short stiff white hairs proceeding from each. As these mamillæ seem to furnish the best characters for discriminating these larvæ from others of the same tribe closely allied to them, it will be necessary to advert to their number and position more narrowly.

There are none on the first segment. On the second, third, and last, are four placed in a transverse line: and on each of the remaining segments, that is, from the fourth to the eleventh inclusive, are six, one on each side, and four in the middle, forming a square, of which the two anterior are larger and nearer to each other than the two posterior. It is to be observed that this

this description applies only to the *back* of the larva, as both the belly and sides have other similar mamillæ, which it is unnecessary to particularize. The period in which these insects exist in the larva state, is, as far as my observations extend, about a year; during the whole of which, except in winter, when they probably lie torpid, they are employed in boring into the bark and alburnum. As the female moth seems to deposit her eggs through the whole summer, the larvæ may be always met with, and of very different sizes.

Chrysalis.—The larvæ which are then full grown, and these are the greater number, assume the state of chrysalis about the latter end of May, soon after which time many of the empty husks from which the moths have escaped, may be seen projecting from the bark: and from this period, to the end of summer, others, lying still undisclosed within their silk-lined cavities, are found on cutting into the wood. The chrysalis has the usual sub-conical shape of those of the tribe of *Tortrices.* It is about one-third of an inch long, and a line broad in the widest part; of a pale yellow colour when first disengaged from the larva, but nearly brown when mature; and smooth, except that each abdominal segment is set with two transverse lines of *aculei,* or little teeth, pointing towards the tail, of which those in the line nearest the head are larger and fewer in number than those in the line next the tail. These *aculei,* which are found in the chrysalis of most species of *Tortrix,* evidently serve for enabling the insect, when in this state, to move itself to the entrance of the orifice in the bark, previously to escaping in its perfect form. The tail, when viewed under a lens, is found to be furnished with seven or eight minute hooks.

Perfect Insect.—After remaining in the chrysalis state about ten days, the moth breaks forth. Of this the following is a description:

TORTRIX WŒBERANA.

T.—Upper wings chocolate-brown, variegated with orange and silver streaks.

Pyralis Wœberana. *Fabricius Mant. Ins.* II. p. 230. *n°* 52. —*Ent. Syst.* III. ii. *p.* 259. *n°* 71.

Tortrix Wœberana. *Wiener Verzeichniss, 4to edit.* 126. 9.; *8vo edit.* II. 43. *Fam. B. n°* 9.—*Haworth Lepidopt. Brit. p.* 457, *n°* 201.

Phalæna Tortrix Wœberana. *Gmelin Syst. Nat.* I. *p.* 2511.—*Turton's Translation,* III. *p.* 350.—*Brahm Insekten Kalender* II. *p.* 252. *n°* [illegible]5.—*De Villers Ent. Linn.* IV. *p.* 525.

Tortrix ...... *Hubner Schmet. Tort.* 32. 6.

De-

Description.—Head brown, margined behind with orange. *Proboscis* short, pale yellow, spirally convoluted between the *palpi*, which are large, subtriangular, yellow, the apex and minute terminal joint black. *Antennæ* one-third the length of the body, setaceous, not pectinate, brown, the first joint, which is thicker than the rest, yellow. *Thorax* brown, with two interrupted irregular transverse bars of orange. *Upper-wings* brown, beautifully variegated with many irregular streaks of orange, and a few of silver. The silver streaks are situate chiefly next the margin: one just above the middle of the wing, anteriorly dividing into a fork, whose ends approach the margin; another below the middle, extending in a curved direction nearly to the apex, and sending off anteriorly two or three branches towards the margin. These silver lines are margined with orange, as are two other short transverse silver lines near the inner angle of the apex of the wing, which include a small silver spot, and two longitudinal orange bars. Besides numerous orange streaks and marks, which it is unnecessary to describe minutely, the wings are characterized at the outer margins by about six short oblique yellow spots. At the apex they are fringed with brown cilia, which in some lights have a metallic shade, and are interrupted by two longitudinal bars of yellow cilia. *Under-wings*, above wholly of a brownish black, except at the outer margin from the base to the middle, where they are white. At the apex they are fringed with cilia, white at the apex, and circumscribed just above the brown base with a very fine and almost imperceptible white line. Under-side of the *body* and *legs* of a silvery or pearly white; the *tibiæ* and *tarsi* of the latter ringed with black. Length of the body about one-third of an inch; of the wings, when expanded, from half to three-quarters of an inch.

Long as the above description may seem, it will not be deemed too minute by any one acquainted with the difficulty of discriminating many of the minuter species of this tribe of insects; nor could I have contracted it consistently with the object I have in view, that of enabling any gardener to recognise the moth in question.

How long these moths live after being excluded from the chrysalis, I am not able to say; but from analogy, and the circumstance that some which I reared under a glass jar did not survive above a week, I conclude their term of existence does not much exceed that period. Hence, as I find them in my garden from May to the middle of August, it is clear that they are not, like many other insects, confined to one term of exclusion, but are issuing from the chrysalis throughout the whole summer: in greater number, however, in June than afterwards. In the day-time they usually remain sitting at rest on the trunks

and branches of the trees from which they have emerged; flying about, like other moths, only in the night. The sexes, judging from those I reared under glass, copulate soon after their exclusion from the chrysalis; and as the female, as remarked by Brahm, is not provided with any instrument for piercing the bark, it is probable that she deposits her eggs on the outside of it, the young larvæ subsequently making their way into the tree.

The only work in which I have found any allusion to the œconomy of this insect, is a German publication, Brahm's *Insekten Kalender*. In this it is briefly observed, that the larvæ winter in the trunks of *apricot-* and *almond*-trees, upon the sap of which they are supposed to live, and to which it is conjectured they are very injurious.

With regard to the best mode of destroying these insects, when their attacks are injurious, I have nothing better to offer than a few imperfect hints. The first and most essential process evidently is, to cut away the edges of the cankers where they are chiefly found, making the wound smooth, and covering it with any composition likely to prevent the moth from depositing her eggs there again. One precaution is necessary, to put into boiling water, or bury at a considerable depth, the cut out pieces of decayed bark containing the larvæ; which, if left near the tree, would soon crawl from their holes and remount it; thus defeating the labour of the horticulturist, who, often, from neglecting a slight additional trouble, loses the benefit of more painful exertions.

Rösell tells us (*Insekten Belustigung*, I. iv. 171) that the German gardeners, after collecting from their cabbages, with unwearied industry, whole baskets full of the destructive *Noctua Brassicæ*, bury them in a shallow hole in the earth; thus unwittingly counteracting their object in the most effectual way. For as this insect naturally undergoes its metamorphosis under ground, and many of the larvæ are full grown, they assume the chrysalis form in the hole into which they have been thrown, and in a few weeks emerge in the moth state, ready to lay thousands of eggs for a new brood.

Where the larvæ are found to have insinuated themselves generally into the rough bark of old trees, it would probably be advisable to adopt Mr. Knight's judicious recommendation on another occasion, and scrape off the whole of the lifeless bark, and such portions of the alburnum as are injured; a process which, there can be no doubt, would be advantageous to the tree in other respects pointed out by Mr. Knight. Where projecting saw-dust-like masses show that the larva has attacked even smooth-barked trees, the insertion of a blunt pricker into the hole would probably, in most cases, suffice to destroy it, and do

less

less injury to the tree than suffering it to attain its growth. But the mode which I should recommend in this, as in the case of almost all insects injurious to the horticulturist, is to employ children in the summer months to destroy the moths themselves, giving a small premium for every ten or twenty they collect; and increasing it as the numbers become lessened. When taught where to look for them, they would discover numbers on the bark of the trees; and, if provided with gauze clasp-nets, would find it a most healthy and interesting occupation to catch them when made to fly by shaking the trees and bushes in which they repose. The destruction of every female moth, before the deposition of its eggs, may be fairly calculated to prevent the existence of some hundreds of larvæ; and thus in any garden not in the neighbourhood of others where the same methods are neglected, the whole race might in a few years be extirpated.

XCI. *On a new Method of determining the Latitude of a Place by Observations of the Pole-Star. By* FRANCIS BAILY, *Esq. F.R.S.**

It is well known that the usual mode of determining the latitude of a place is by observing the zenith distance, or the altitude, of the sun or certain of the fixed stars at the precise moment of their passing the meridian. But, although this point of time might appear the most favourable for such observations, yet, by the assistance of a refined analysis, the *modern* astronomer has been enabled to extend the period of such observations to several minutes on each side of the meridian; and the results are, by the help of certain tables constructed for that purpose, rendered as correct as those which are taken immediately at the time of culmination.

Of all the stars which have been observed for this purpose, none have engrossed so much the attention of astronomers, as the *pole-star:* and, in fact, it is almost the only one which is now resorted to on such occasions. Its proximity to the pole renders it highly favourable for such observations in this hemisphere; it being on that account, and from its magnitude, visible (with telescopes of no very considerable power) at all times of the year, by day as well as by night.

But there is another important advantage to be derived from its small polar distance, which, till of late years, appears to have escaped the attention of astronomers; and which it is my object, in the present communication, to point out to those who may not have considered the subject: viz. that the latitude of a

* Communicated by the Author.

place

place may be determined with the greatest accuracy, by observing the altitude of the pole-star, *at any part of its diurnal revolution.*

In the *Zeitschrift für Astronomie*, vol. iii. page 208, published in 1817, M. Littrow has drawn the attention of astronomers to this subject; and given a formula for determining the correct latitude of the place from the observed altitude of the pole-star, at any hour of the day or night. He has afterwards pursued the subject in various other publications, and in the *Correspondance Astronomique* of M. Zach, vol. iv. page 370, he has the following passages: " From time immemorial we have been contented with taking the meridian altitude of the pole-star at the two moments of its passing the meridian in 24 hours. These two points are, without doubt, the most favourable for determining the latitude of the place of observation, independent of the declination of the star. Of late years, it has been proposed to take the altitude of this star at the time of its greatest elongation from the meridian, either east or west: but these two points are still less favourable, particularly if the time is not determined with the greatest rigour; and it appears to me that every other point of the parallel of this star is preferable to these two, as I shall here endeavour to demonstrate." M. Littrow then inserts a formula for this purpose, which is the same as that given in the *Zeitschrift für Astronomie:* but which is different from that to which I shall presently allude. He afterwards proceeds thus:

" We see, by this, that an error in the observed *zenith distance* produces every where nearly the same error in the latitude: which is also the case in the meridional passages; and which therefore, in this respect, have no preference over any other part of the parallel of this star. As to the error in *north polar distance*, there is little to fear on this point, since the position of the pole-star is now well determined: and moreover the error in latitude which would result therefrom is even less in every other point of the parallel than in the meridional passages, which thus appear the least advantageous. Let us now consider the error in the *time*. It is true that this error does not influence the observations made on the meridian; and therefore they may seem preferable to any other. But it is easy to see that an error in time has a very trifling effect on the latitude in any part of the parallel. Suppose the error in time to be one second (or 15 seconds in arc) we shall have for

the horary angles	..	6^h	4^h	2^h
the errors in latitude	..	$0'',4$	$0'',3$	$0''2$.

Every practical astronomer knows that an error of $0'',4$ in arc is inappreciable, that it is impossible to answer for it in our largest

largest circles and with the most perfect instruments. Besides, we have always the means of eliminating this error: we have only to take the altitudes at an equal distance on the opposite side of the meridian, and the error destroys itself."

"We see therefore, in every view of the case, that in point of accuracy, *it is indifferent whether we observe the pole-star at the time of its culmination, or at any other point of its parallel.* But for the convenience of the observer, and the infinite advantage of being able to collect in a short time an immense number of good observations for the latitude of the place, this latter method seems preferable to all others. The observer does not depend on a single instant, which presents itself only once in 12 or 24 hours, and of which he may be deprived by the weather, a passing cloud, or some other untoward circumstance. By the method here proposed he may at any time he thinks proper take the altitude of the star, by day or by night, if the weather permit, and if he have time and opportunity: and he can thus in 24 hours make as many observations as he pleases, and collect in this interval a great number of latitudes."

The advantages thus detailed by M. Littrow must be evident to every practical astronomer; and have since engaged the attention of several mathematicians. In Bode's *Astronomische Jahrbuch* for 1823, Dr. Dirksen has also given a formula for deducing the latitude from such observations. And in M. Zach's *Correspondance Astronomique,* vol. v. page 308, Mr. Horner has deduced another formula for the same purpose. In vol. vi. page 71, of the same work, M. Littrow asserts that the formula of Dr. Dirksen is erroneous, and that Mr. Horner's might be much simplified. He also states that it appears that M. Schumacher has calculated his tables for finding the latitude by the pole-star, inserted in his Ephemeris of the planets, from a formula similar to that of Dr. Dirksen. Indeed, the argument not only of one of M. Schumacher's tables, but also of Mr. Horner's, is the *latitude of the place;* which is, in fact, the very quantity *sought.* This remark will probably attract the attention of these astronomers, and induce them to revise their formulæ. The formula, ultimately adopted by M. Littrow, is investigated as follows:

"Let p be the apparent polar distance of the star, z the observed zenith distance of the star, t the horary angle of the star, or the sidereal time elapsed since the moment of its upper culmination, and ψ the height of the equator or the co-latitude of the place: and make x (= the correction) equal to $\psi - z$, Then, by spherical trigonometry, we shall have

$$\cos z - \cos p . \cos (z+x) - \sin p . \sin (z+x) . \cos t = 0.$$

By

By reducing the sine and cosine of $(z+x)$, and putting

$$\sin x = \frac{2 \tan \frac{x}{2}}{1+\tan^2 \frac{x}{2}} \text{ and } \cos x = \frac{1-\tan^2 \frac{x}{2}}{1+\tan^2 \frac{x}{2}}$$

we shall, by neglecting the fourth and fifth powers (which we may safely do) have

$$\begin{aligned} \tan \frac{x}{2} = & \tfrac{1}{2} \sin p. \cos t \\ & - \tfrac{1}{4} \sin^2 p. \sin^2 t. \cot z \\ & + \tfrac{1}{8} \sin^3 p. \cos t. (1 + \sin^2 t) \end{aligned}$$

whence, by a very simple transformation, we have *the correction*,

$$x = p. \cos t - \tfrac{1}{2} p^2. \sin^2 t. \cot z + \tfrac{1}{3} p^3. \sin^2 t. \cos t$$

which is the expression required: and which, for all observations of the pole-star, is correct as far as p^4. This formula is very easy to calculate: and still more easy to reduce into tables."

M. Littrow then goes on to state that, for a *fixed observatory*, we may put into *one* table the last two terms of this expression, for all the values of t; viz. by making $A = \frac{p^2}{2} \sin^2 t. \cot z - \frac{p^3}{3} \sin^2 t. \cos t$. Whence we shall have $\psi = z + p. \cos t - A$. But, he seems to have forgotten that z also is variable; which would render a table of this kind (even for a fixed observatory) more extensive than is necessary. The most convenient mode of arranging such tables appears to be as follows:

$$B = \frac{p^2}{2} \sin^2 t$$

$$C = \frac{p^3}{3} \sin^2 t$$

whence we shall have

$$\psi = z + (p + C) \cos t - B \cot z.$$

In order to render this method more convenient in practice, I have computed the values of B and C for every ten minutes; which are inserted in the two small tables annexed: and which will be found to be somewhat different from those given by M. Littrow. I have assumed the north polar distance of the star to be 1° 38′: but, as the mean north polar distance of this star is constantly decreasing, I have subjoined the *decimal*, by which the quantities in the tables must be multiplied, when the star's apparent north polar distance is at any of the points there stated. The observer will adopt or reject these corrections, according to the degree of accuracy required.

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For

For *nautical* purposes, the correction has been sometimes assumed equal to $p.\cos t$ only: and in this manner it has been inserted in several books of navigation. But it would be more proper to add M. Littrow's second term $\frac{p^2}{2}.\sin^2 t.\cot z$. His last term is wholly insensible at sea.

The time denoted by t is the horary angle of the star, or the sidereal time elapsed since the star passed the meridian. If S denote the correct sidereal time at which the observation was made, and Æ denote the apparent right ascension of the star on the day of observation; then will $t = (S - Æ)$: noting always to increase S by 24^h, if it should be *less* than Æ. The apparent right ascension and north polar distance of the pole-star, for every tenth day of the year, may be obtained from the Nautical Almanac.

I shall now subjoin an example of the use and application of these tables. Let $p = 1°.\,38'$; $z = 39°.\,12'.\,16'',4$; and $t = 4^h = 60°$.

$p =$	$1°.38'.0'',00$		
$C =$	$1,19$	*Logarithm.*	
	$1.38.\ 1,19$	$= 3.7694653$	
	cos $60.\ 0.\ 0,00$	$= 9.6989700$	
	$0.49.\ 0,59$	$= 3.4684353$	
$z =$	$39.12.16,40$		
	$40.\ 1.16,99$		
	$-\ 1.17,05$		
$\psi =$	$39.59.59,91$		

		Logarithm.
$B =$	$0°.1'.2'',86$	$= 1.7983052$
cot	$39.12.16,40$	$= 10.0884646$
	$1.17,05$	$= 1.8867698$

Which is the same as the value deduced from the rigorous formula,

$$\tan u = \tan p.\cos t$$

$$\cos(\psi - u) = \frac{\cos u.\cos z}{\cos p}.$$

M. Littrow has stated that the value of M, which he has deduced from the term $\frac{p}{2}\sin^2 t$, must be multiplied by 1·02 for every increase of one minute in the north polar distance of the star. This is true, within certain limits; but he has neglected to state that his value of N ought, for the same reason, to be multiplied by 1·03, in order to obtain the correct values. The trifling difference which exists between our results in the preceding example, is principally owing to this slight correction.

TABLE I.

Argument = t		B	C	Argument = t	
	′	″	″	′	
0^h. and 12^h.	0	0,000	0,000	60	11^h. and 23^h.
	10	0,159	0,003	50	
	20	0,637	0,012	40	
	30	1,428	0,027	30	
	40	2,527	0,048	20	
	50	3,926	0,075	10	
1^h. and 13^h.	0	5,614	0,107	0	
	10	7,575	0,144	50	10^h. and 22^h.
	20	9,804	0,186	40	
	30	12,274	0,233	30	
	40	14,969	0,284	20	
	50	17,869	0,340	10	
2^h. and 14^h.	0	20,958	0,398	0	
	10	24,195	0,460	50	9^h. and 21^h.
	20	27,573	0,524	40	
	30	31,059	0,590	30	
	40	34,628	0,658	20	
	50	38,253	0,727	10	
3^h. and 15^h.	0	41,904	0,796	0	
	10	45,557	0,866	50	8^h. and 20^h.
	20	49,182	0,935	40	
	30	52,751	1,003	30	
	40	56,237	1,069	20	
	50	59,615	1,133	10	
4^h. and 16^h.	0	62,857	1,195	0	
	10	65,941	1,253	50	7^h. and 19^h.
	20	68,841	1,308	40	
	30	71,536	1,360	30	
	40	74,006	1,407	20	
	50	76,231	1,449	10	
5^h. and 17^h.	0	78,196	1,486	0	6^h. and 18^h.
	10	79,883	1,518	50	
	20	81,282	1,545	40	
	30	82,382	1,566	30	
	40	83,173	1,581	20	
	50	83,650	1,590	10	
	60	83,810	1,593	0	

TABLE

TABLE II.

Corrections of B and C.

Argument = N.P.D. of the Pole-Star	Multiplier of	
	B	C
1°. 37′. 50″	·997	·995
40	·993	·990
30	·990	·985
20	·986	·980
10	·983	·975
1. 37. 0	·980	·970
1. 36. 50	·976	·965
40	·973	·960
30	·970	·955
20	·966	·950
10	·963	·945
1. 36. 0	·960	·940
1. 35. 50	·957	·935
40	·953	·930
30	·950	·926
20	946	·921
10	·943	·916
1. 35. 0	·940	·911

XCII. *Experiments on the Combination of Acetic Acid and Alcohol with volatile Oils.* By M. VAUQUELIN *.

Exp. 1.—EIGHTY parts of volatile oil of lavender were mixed with 80 parts of acetic acid; the areometer at 10°. After shaking the mixture smartly for a considerable time to perfect the union of the two liquids, it was left to settle. When again examined and separated, the oil was found to occupy 125 parts, and the acid only occupied 35; the latter had therefore lost 45 parts, and the oil had acquired 45 parts.

Exp. 2.—Eighty parts of the same oil were joined to the 35 parts of acetic acid remaining, and after mixing and separating as in the preceding experiment, the oil was found to occupy 115 parts, and the acid was reduced to 5: so that this time the 80 parts of oil absorbed only 30 parts of acid.

* From the *Annales de Chimie* for March 1822.

 I think

I think that if the oil has only absorbed this time 30 parts of acid in place of 45, it is owing probably to the acid having become more aqueous, and therefore less fit for mixing with the oil. Of the 100 parts of acetic acid employed for this experiment, there were six parts which could not combine with the oil. This remnant of the acid had acquired a yellow colour: its taste was still very acid, and its odour indicated that it contained much oil. In fact, when a drop of this acid was put in water, it fell to the bottom, and the oil separated and mounted to the surface.

In this experiment, the acetic acid and oil formed two compounds of unequal proportions; one in which there was an excess of oil, the other in which there was more acid than oil. It appears from this experiment, that 100 parts of oil of lavender can absorb 56 parts of acetic acid; but as the portion of vinegar which remains, holds in solution a certain quantity of oil not easy to be estimated, it may be concluded that 50 parts of vinegar will saturate 100 of oil, that is to say, one portion of acid for two portions of oil.

Exp. 3.—To know if water could separate acetic acid from oil, 50 parts of the compound richest in oil, and 55 parts of water, were well shaken together for a long time. It was then found that the bulk of the oil was reduced to 35, while that of the water had been augmented 15: the oil however was still acid; in fact, it contained three parts of acetic acid.

Twenty parts of the same compound were shaken with 80 parts of water; the oil on settling had lost eight, and the water was augmented in the same proportion. In this experiment, the water had abstracted from the oil the whole of the acid which it contained, and had absorbed also a little oil, since the 20 parts of the compound contained but 7·2 of acid, and there was a loss of 8.

When the acetic acid is pure, the oil can absorb it entirely; but if it contains a portion of water, were it only 5 per cent., a part will remain which the oil cannot seize upon; so that the part of the acetic acid which does not combine with the oil, contains necessarily a greater quantity of water than vinegar previous to the operation.

This property which vinegar possesses, of combining with volatile oil, ought not to cause any surprise, for it is well known with what facility this acid imbibes the odours of plants.

Effects nearly similar are produced when camphor is dissolved in nitric acid, and also in acetic acid; that is to say, the camphor seizes on the pure part of the acids, and leaves another watery portion which was previously combined with the whole of

of the acid. The greater the quantity of camphor, the less is the portion of acid remaining with the water; the latter contains also a small quantity of camphor, but that which water cannot separate: this quantity of camphor ought to be nearly the same as that which remains in the acidulated water, when oil of camphor is decomposed by water.

These effects are not confined to greasy bodies and to acids; they occur equally between alcohol and these same greasy bodies.

Having long ago been applied to by the *Regie des Octrois de Paris*, to know whether it be possible to introduce under colour of essences, turpentine for example, a certain quantity of alcohol (a fraud which can only be effected by the manufacturers of varnishes), I made on this subject some experiments, which proved to me that a certain quantity of alcohol can be mixed with essences, without our being able to detect it by the ordinary means, because, as long as the bulk of alcohol does not exceed that of oil, the mixture or combination will not be disturbed by water, and the odour will be masked by that of the essence which is strongest.

I have repeated lately some of these experiments; the following are the results:

Exp. 1.—100 parts of volatile oil of turpentine and 20 parts of alcohol mixed together, did not separate on being left to settle. and formed a homogeneous body: this effect is produced by the solution of the alcohol in the oil; for one portion of alcohol cannot dissolve five parts of oil.

Exp. 2. —The above mixture, shaken for a long time, and at intervals with water added, was reduced to 108. The water had therefore abstracted 12 parts of alcohol from the oil, and the oil had preserved 8.

Oil of turpentine may therefore contain a twelfth of its bulk of alcohol without its being liable to be perceived, unless it be through the specific gravity, which is a little diminished: however, if the lotions are repeated enough, the whole of the alcohol may be at last separated from the oil.

The mixture or combination of 100 parts of oil of turpentine, and 20 parts of alcohol, is not disturbed by water; but when poured upon water and slightly agitated, a portion of the alcohol will be seen to detach itself, and to form, in uniting with the water, some very marked streaks.

XCIII. *No-*

XCIII. *Notices respecting New Books.*

Recently published.

Travels in Syria and Mount Sinai. By the late John Lewis Burckhardt; viz.— I. A Journey from Aleppo to Damascus. —2. A Tour in the District of Mount Libanus and Antilibanus. —3. A Tour in the Hauran.—4. A Second Tour in the Hauran. —5. A Journey from Damascus, through Arabia Petræa, and the Desert of El Ty, to Cairo.—6. A Tour in the Peninsula of Mount Sinai, with a Map. 4to. 2*l*. 8*s*.

Remarks touching Geography, especially that of the British Isles. By Mela Britannicus. 8vo. 10*s*. 6*d*.

Travels along the Mediterranean, and Parts adjacent, extending as far as the second Cataract of the Nile, Jerusalem, Damascus, Balbec, Constantinople, Athens, Joannina, the Ionian Isles, Sicily, Malta, and Naples, in the years 1816, 1817, and 1818, in company with the Earl of Belmore. By Robert Richardson. 2 vols. 8vo. 1*l*. 4*s*.

The British Gallery of Pictures, selected from the most admired productions of the Old Masters in Great Britain: with descriptions, &c. By the late Henry Tresham, R.A. and W.Y.Ottley, Esq. F.S.A. 4to. 12*l*. 12*s*. extra boards; proofs India paper, 25*l*. 4*s*.; coloured in imitation of the original pictures, 151*l*. 4*s*. in Russia.

Engravings of the Marquis of Stafford's Collection of Pictures. With Remarks, &c. 4 vols. 4to. 35*l*. 14*s*. bds.; proofs, 71*l*. 8*s*.; finely coloured, &c. 178*l*. 10*s*.

An Inquiry into the Principles of Beauty in Grecian Architecture; with an Historical View of the Rise and Progress of the Art in Greece. By George, Earl of Aberdeen, K.T. &c. Post 8vo. 7*s*. 6*d*.

A System of Mechanical Philosophy. By the late John Robison, LL.D., Professor of Natural Philosophy in the University, and Secretary to the Royal Society, of Edinburgh. By David Brewster, LL.D., F.R.S.E. Four exceedingly large and closely-printed volumes, 8vo. with Notes and 30 Plates. 4*l*.

A new Theory of the Tides; showing what is the immediate Cause of the Phænomenon; and which has hitherto been overlooked by Philosophers. By Captain Forman, R.N. 8vo.

A Monograph on the British Grasses. By George Graves, F.L.S. No. II. 4*s*. 6*d*. and 6*s*.

Lectures on the Elements of Botany. Part I. By Anthony Todd Thomson, F.L.S. 8vo.

We have been dilatory in noticing the appearance of a THIRD VOLUME, as an APPENDIX (in *two* Parts) to Mr. Purton's valuable

able MIDLAND FLORA. We cannot withhold our acknowledgement of the faithful industry and botanical accuracy displayed by its respectable Author. This third volume, Price 1*l*. 10*s*., is embellished with thirty coloured Engravings.

Preparing for Publication.

A History of a severe Case of Neuralgia, commonly called *Tic Douloureux*, occupying the Nerves of the Right Thigh, Leg and Foot, successfully treated; with some Observations on that Complaint, and on its Causes as they vary in different Individuals. By G. D. Yeats, M.D. F.R.S. Fellow of the Royal College of Physicians, &c. &c.

A Treatise on the Morbid Respiration of Domestic Animals, illustrative of the Diseases of the Organs of Respiration in Horses, Cows, Sheep, and Dogs, with the most approved Methods of Treatment, including a variety of Cases and Dissections. By Edward Causer, Surgeon, late Veterinary Surgeon to His Majesty's 4th Regiment of Dragoons.

XCIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 25.—ON the Mechanism of the Spine, by Mr. Earle.

Observations on the Eclipse of August 1821, by Mr. Dawes.

May 2.—On the Nerves which associate the Muscles of the Chest in the Actions of Breathing, Speaking, and Expression, by Charles Bell, Esq.

A short Account of some Appearances in the Moon on the 24th of April, by Mr. Lawson.

May 9.—Experiments and Observations on the Newry Pitchstone, and on the artificial Formation of Pumice, by the Right Hon. J. Knox.

May 16.—On the Changes which the Egg undergoes during Incubation, by Sir E. Home, Bart.

May 23.—On the Mathematical Laws of Electro-magnetism, by P. Barlow, Esq.

On the Heights of Places in the Trigonometrical Survey. by B. Bevan, Esq.

ASTRONOMICAL SOCIETY OF LONDON.

Friday, June 14.—Two papers were read from Mr. Gompertz: one on A new method of investigating the formulæ for expressing the Aberration of Light: the other being a continuation of a former paper on the means of correcting the errors of astronomical instruments by certain mathematical expressions.

A paper

A paper was also read from Mr. Bevan on the practicability of determining the difference of longitude between two places not far distant from each other, by observing flashes of lightning. It seems to us, however, that the mode adopted on the Continent, by firing loose gunpowder at certain intervals, is much preferable.

A paper was likewise read from M. Littrow of Vienna, pointing out certain anomalies in the north polar distance of the principal fixed stars. It appears that the instruments recently made by Reichenbach, for the observatories of Konigsberg, Vienna, Gottingen, &c. give the north polar distances of those stars greater, by 4 or 5 seconds, than the larger instruments at Greenwich, Dublin, Palermo, &c. This subject is one of great interest at the present moment: and we wait, with much impatience, the publication of this memoir.

The next and last paper was by Mr. Babbage, relative to a new invention in machinery, by which not only the usual logarithms, but also various other mathematical and astronomical tables might be formed, and the *types* thereof *set up* without the *possibility* of an error throughout the whole process. This discovery we consider as one of the finest in modern times; and (like the invention of the steam-engine in its application to the arts) it bids fair to open a new era in science. The object, which Mr. Babbage had first in view, was to form an engine which should express any series of numbers whose first, second, third, &c. differences were equal to 0: and which he has completely effected. To those who are acquainted with the method of differences, it will be evident that such series would embrace not only the common logarithms of numbers, the logarithms of sines, tangents, &c.; but likewise the natural sines, tangents, &c. whence its application to the formation of astronomical tables may be readily conceived. But, in the pursuit of this inquiry, Mr. Babbage found that many new views of the subject arose; and that the engine was not confined to the expression of series whose ultimate differences were constant: but that it would form tables, not dependent on that law; and whose differences could *not be denoted by any analytical expression.*

Since the meeting of the Society we have been favoured with a sight of this engine: which is very simple in its construction, and may be put in motion by a child. Mr. Babbage composed in our presence a long series of square numbers; and likewise the first forty terms of the series of numbers depending on the formula $(x^2 + x + 41)$; all of which are *primes*, and which were formed as expeditiously as a person could write them down. When we consider the numerous errors to which all tables are liable, not only in their original computation, but also in composing them

them for the press, as well as the various accidents to which the type is exposed in passing through the hands of the various workmen (*all* of which sources of error are *completely* obviated by the present contrivance); we cannot sufficiently congratulate our readers on so valuable an acquisition, which we trust will be secured to *this country* by the liberal protection and support of our own Government to the ingenious inventor.

Mr. Hardy exhibited a time-piece for determining very small portions of time. It showed the $\frac{1}{100}$th part of a second of time. M. Patton (nephew of the celebrated Breguet) exhibited also a new species of chronometer, which, by means of a mechanical contrivance annexed to the seconds hand, marked on the dial-plate the fractional part of a second: thereby affording to observers an opportunity of registering, during an observation, the precise moment at which any phænomenon may occur.—Mr. Robinson likewise exhibited his micrometer with two fixed spider lines, and one moveable line: which for œconomy and accuracy is worthy the attention of every practical astronomer.

The Society adjourned, for the summer recess, till Friday, November 8th.

ROYAL ACADEMY OF SCIENCES OF PARIS.

At the sitting of this Academy on the 7th of January, M. G. St. Hilaire read a paper entitled "Observations on different encephalic parts found in human monsters reputed to be without the brain, and named after this hypothesis Anecephales." M. Desmarets read a memoir "On the Crustaceous Fossils." M. Ampere, one "On the rotation of a Magnet, which only turns upon its axis by the action of a metallic wire joining the two extremities of a Voltaic pile," a phænomenon which Mr. Faraday had in vain attempted to produce. On the 14th of January M. Fourier read a memoir "On the general principles of Algebraic Analysis." M. Brongniart the younger read a paper upon "the classification and the distribution of vegetable Fossils in general." M. G. St. Hilaire, a Memoir "sur les voies digestives des monstres acephales." On the 22d, M. Gaetano Rosina read a paper "On azote, carbon, and hydrogen, rendered solid by the means of oxide of iron; with a bottle containing a specimen of the substance." M. G. St. Hilaire read a memoir "On the intestinal nutrition of the fœtus, and on its great conformity with the intestinal nutrition in adult animals." M. Cauchy, one "On developments in series, and on the integration of differential equations." On the 28th M. Laugier presented a paper on the aërolithe of Juvenas, and an extract was read from a work of M. Reboul on the Pyrenees. M Delambre gave a verbal account of the new part of the translations of M. Halma.

Prizes proposed by the French Royal Academy of Sciences for 1823.

In Physics.—The origin of animal heat is not established in an incontestable manner, and philosophers are still divided in opinion on the subject, notwithstanding its great importance to the progress of physiology.

The Academy offers a gold medal of the value of 3000 francs, to be awarded in the Public Sitting of the year 1823, for the best Treatise founded on actual experiments, On the causes, whether chemical or physiological, of Animal Heat? It is particularly desired to know exactly the degree of heat emitted by a healthy animal in a given time, and the carbonic acid which it produces in respiration; and what proportion such heat bears to that produced by the combustion of carbon in the formation of the same quantity of carbonic acid. The Essays to be transmitted to the Secretariat of the Institute before the 1st of January 1823.

In Mathematics.—The Academy, persuaded that the theory of Heat is one of the most interesting objects to which mathematics can be applied, propose the following question for a Prize to be awarded in March 1824.

1. What is the density, as proved by experiments, which liquids, especially mercury, water, alcohol, and sulphuric ether, acquire, by degrees of compression equivalent to the weight of so many atmospheres?

2. How to measure the effects of the heat produced by these compressions?

The Prize to be a gold medal of the value of 3000 francs. The Essays to be transmitted before the 1st of January 1824.

Prizes founded by M. Alhumbert.

The late M. Alhumbert having bequeathed an annuity of 300 francs to be employed in promoting the sciences and arts, the King has authorized the Academies of Sciences and Fine Arts to distribute alternatively every year a Prize of that value.

The Academy proposes the following subject for the competition of this year—

To compare anatomically the structure of a fish and that of a reptile; the two species to be chosen by the competitors.

Prize of Experimental Physiology founded by M. de Montyon.

The Prize a gold medal of the value of 895 francs, for the work, printed or in MS., which shall appear to have contributed most to the progress of experimental physiology.—To be sent to the Secretariat of the Institution before the 1st of January 1823.

Prize

Prize in Mechanics founded by M. de Montyon.

To the person who shall have shown the greatest merit in inventing or in improving instruments useful to the progress of agriculture, mechanical arts, and sciences, a gold medal of the value of 1500 francs.

The prize will only be given to machines the description and the plans or models of which, sufficiently detailed, shall have been submitted to the Academy, either separately, or in some printed work transmitted to the Academy.

SOCIETY OF MEDICINE AT PARIS.

This Society has offered two prizes for 1822 and 1823, for the best paper on the following subjects:—On the symptoms, the causes, and the treatment of the malady known by the name of the cerebral or hydrocephalic fever. 2. The morbid alterations of which traces are found in the abdominal viscera, are they the cause or effect, or the complication of these diseases?

ASIATIC MUSEUM AT ST. PETERSBURG.

His Excellency the President of the Imperial Academy of Sciences at Petersburg, has ordered all the researches and resources of Eastern learning, that can be obtained, to be collected together, and placed in one of the rooms of the Academical Museum. He has by these means formed an Asiatic Museum, which has been enriched by Imperial liberality with a new collection of Oriental MSS., and, in other branches, by presents from individuals, forming now one of the most useful and remarkable collections in the Academical Museum. It has been arranged in three newly-erected rooms, and contains:

I. Oriental monuments and antiquities: 1. A large collection of Mohammedan coins, divided into 28 classes; a complete catalogue of which is now in the press, and of which a particular account will shortly be given. 2. A collection of other Oriental coins, such as Chinese, Japanese, Hebrew, Sassanide, and Indian. 3. Other Oriental antiquities, as stones (bricks) with Persepolitan inscriptions; and vessels with Arabic inscriptions.

II. A very fine collection of Arabian, Persian, and Turkish MSS., arranged according to their different departments and languages: as poems; grammars; mathematical, historical, philosophical, physical, and theological MSS.

III. A rich collection of Chinese, Manshurian, and Japanese MSS., likewise arranged according to languages and subjects; to which are added Chinese sketches and drawings.

IV. A very rare collection of Mongol, Calmuck, and Tibetian MSS.;

MSS.; also many Mongol prints, a detailed catalogue of which will be published, to satisfy the curiosity of the public.

V. An Oriental library, or a collection of Oriental MSS. relative to literature and information, which may furnish the learned with sufficient means to obtain a knowledge of the countries of the East.—*New Monthly Mag.*

XCV. *Intelligence and Miscellaneous Articles.*

ETHIOPS MINERAL.

Dr. Taddei recommends the following process for the preparation of ethiops mineral, as being one which effects the combination immediately, and in a more perfect manner than that generally employed. Put one part of sulphuret of potash into a mortar, with three or four parts of running mercury; triturate together, adding a little water by degrees, until the whole is reduced to a homogeneous black paste; then add flowers of sulphur in equal quantity to the mercury employed, and mix the whole by a short trituration. Then wash the whole; and filter with repeated portions of water, till all the alkaline sulphuret is removed. Ethiops thus prepared is not of the black colour of that obtained by simple trituration, but it is a more perfect combination. Dr. Taddei says, that the addition of a little sulphuret of potassa to the mixture of sulphur and mercury does not do away with a long trituration, but that, proceeding as above, the substance is prepared instantly.—*Giornale di Fisica.*

NEW METHOD OF MANUFACTURING SALTPETRE.

M. Baffi, the celebrated chemist, a native of Pergola, has received from the Viceroy of Egypt a present of 100,000 crowns and the title of Bey, for having discovered a method of producing saltpetre without the assistance of fire, by mere heat of the sun. Previous to this, every hundred weight of saltpetre cost the Viceroy ten crowns, which is reduced to one crown by the new method. The manufactory erected by M. Baffi in the great square of Memphis, has furnished within the last year 3580 cwt. of saltpetre. An Egyptian cwt. is the same as the English.

GAS FROM COAL TAR.

It has been found, by experiment, that the coal-tar liquor, which is sometimes considered as waste by those who make gas, if mixed with dry saw-dust, exhausted logwood, or fustic, to the consistence of paste, and allowed to remain till the water has drained

drained off,—2 cwt. of the mass being put into the retort instead of coal will produce more gas, and be less offensive than the same quantity of canal coal. This process will probably be found very convenient in some circumstances for the consumption of the tar produced by the distillation of coal in gas-works.—*Journal of Science.*

CAPACITY OF THE GASES FOR CALORIC.

J. H. Mallet, secretary to the Academy of Lyons, announces, as the result of some well contrived experiments, which he has published, that at the same temperature the particles of different gases are at equal distances; that their molecules have different volumes; and that the quantity of caloric which a gas can admit depends on the space which separates the molecules.

ON WATER-CEMENTS, MORTAR, AND LIME.

The following particulars are extracted from the work of M. L. J. Vicat on this subject.

Limestones vary greatly in quality. Those which approach to marble in purity, or consist almost entirely of carbonate of lime, are called *rich*; those on the contrary are called *meagre*, which contain notable portions of sand or silex, alumine and iron. The former when burned, slacked, and made into paste, will retain their softness for ages under water, or excluded from the air; but exposed to the air, they contract a remarkable hardness by the double effect of desiccation, and union with the carbonic acid of the atmosphere. They even become susceptible of a beautiful polish.

But the meagre limestones, in general, treated in the same manner, if kept under water, harden in a few days, and at length form a kind of freestone which could be acted upon or broken only by the pickaxe. Exposed to the air, it acquires a crumbly consistence, and will never admit of polish. From this circumstance, the lime which possesses the quality last mentioned is called *hydraulic lime.* But some of the meagre limestones are unfit for hydraulic purposes, especially those which contain large particles of silex.

Puzzolanas are either natural or artificial. The natural is found in situations which have been acted upon by subterraneous heat. They all consist of silex, alumine, oxide of iron, and a little lime, the properties of which vary greatly. Silex is always the predominating ingredient; the lime and iron are sometimes, though rarely, wanting. The scoria of forges and furnaces, broken pottery, and pulverized brick or tile, are artificial substances analogous to puzzolanas.

There is one class of puzzolanas which dissolve readily in sulphuric

phuric acid, and abandon the silex, which immediately subsides. Others resist the action of this acid.

If we mix in various proportions, very rich lime slacked in the usual way, with sand alone, or with puzzolana which resists the action of sulphuric acid, we obtain a mortar, which, placed under pure water, remains always soft, or acquires, after a long time, only a feeble consistence. The same mortar exposed to the air, soon hardens by drying, but is always easily broken or pulverized.

But if the same experiment is made with a puzzolana readily affected and decomposed by sulphuric acid, a mortar is obtained, which soon *sets* under water, and becomes gradually harder, but in air it does not acquire any great resistance in consequence of its drying too rapidly.

Hydraulic lime presents phenomena nearly the reverse. That is to say, it furnishes good mortar when combined with sand alone, or with puzzolana unaffected by acids, whilst very unsatisfactory results are obtained by employing it with substances which unite well with rich or pure lime.

Since the quality of natural hydraulic lime depends only on the presence of a certain quantity of clay or argill combined by heat with calcareous matter, it is natural to suppose that in mixing clay in suitable proportions with a rich slacked lime, and submitting the mixture to heat, the same result might be obtained. Experiments made upon a large scale, and in various places, have confirmed this opinion so fully, that it is now possible to fabricate almost every where, and at a very moderate price, artificial lime superior to the natural.

ELECTRO-MAGNETIC EXPERIMENT.

The discovery of M. Zamboni concerning the electricity produced by the contact of a single solid conductor with a single fluid conductor has been confirmed by Professor Oersted of Copenhagen, who thus expresses himself in a letter on the subject:

"You know that such experiments are extremely delicate; they seem to have been repeated only by Mr. Erman, and this celebrated and ingenious philosopher complains much of the irregularities which the experiments present. I have found in the electro-magnetic multiplier, invented by Schweigger, a mode of making these experiments with the greatest ease. I take two plates of zinc, of different breadths, one for example 3 lines broad, the other 1½; I place them in a diluted acid, and I make a communication between each of them and one of the extremities of the metallic wire of the multiplier. The action is thus rendered very sensible. The wider plate assumes in this galvanic arc the place of the copper, the narrower that of the zinc. When we take two plates which are perfectly equal, and attach them

to the extremities of the multiplying wire, we obtain no effect, if we plunge both plates at once into the liquid; but if we immerse one before the other, that which has come the last into contact acts as a less oxidable metal.—*Journal of Science*, No. 25.

RUSSIAN EMBASSY TO BUCHARIA.

The Russsian embassy sent in 1820 to Bucharia, after crossing, in 72 days, the Kirgese Desert (Steppe), where it suffered many hardships, especially for want of water, reached Bucharia on the 20th of December 1820. They found Bucharia to be a very fruitful and well cultivated country, with two millions and a half of inhabitants. The trade with Russia amounts to twenty millions of rubles.—The embassy set out on its return to Orenburgh on the 23d of March 1821, and arrived there safely in fifty-five days.—*Asiatic Journal.*

ANTIQUITIES.

A monastic seal, in perfect preservation, was found last November in a potatoe field called Low Garth, near Langrick on the Ouse. It is of mixed or bell-metal, 2½ inches long, of an oval shape, pointed at the ends, and pierced through the shaft: the inscription is "SIGILLUM FRATERNITATIS MONASTERII BEATE MARIE DE HAYLES." In the centre, on a ground of flowers, is the figure of a man, clothed in a monkish stole, bare-headed and shorn, standing on an elevation of three steps; holding in his right hand a globe surmounted by a cross, and in his left a staff or sceptre spreading into three rods or branches at the top. Although found within a short distance of Drax Abbey, which was sometimes called also Heilham, and possessed a neighbouring estate named Hales, it cannot be referred to that foundation, which was a Priory dedicated to St. Nicholas; neither does it appear to belong to Hales Owen Abbey, but to the mitred Cistercian Abbey of Hayles in Gloucestershire, which was founded by Richard Earl of Cornwall and King of the Romans, in 1246, at the expense of 10,000 marks, and dedicated to the Virgin. How it came into Yorkshire must be mere conjecture, as there was no connexion between the two establishments.

ROME.—On the 7th of February, a *Columbarium*, in perfect preservation, with beautiful paintings and 200 inscriptions, was discovered in the Vigna Ruffini on the Via Nomentana. Among the inscriptions, one only belongs to a person of the age of eighty. (*Vixit Annos* LXXX.) Friends have scratched their names on the monument, which therefore furnish a remarkable addition to the specimens of Roman running hand. The proprietor means to leave the whole as it was found, and to build a shed over it.

PUFF

PUFF ADDER.

The venom of this reptile is said to be very fatal, taking effect so rapidly as to leave the person who has the misfortune to be bitten, no chance of saving his life but by instantly cutting out the flesh surrounding the wound. "Although," says Mr. Burchell, "I have often met with this serpent, yet, happily, no opportunity occurred of witnessing the consequences of its bite; but from the universal dread in which it is held, I have no doubt of its being one of the most venomous of Southern Africa. There is a peculiarity which renders it more dangerous, and which ought to be known by every person liable to fall in with it. Unlike the generality of snakes, which make a spring, or dart forward, when irritated, the Puff Adder, it is said, throws itself backward; so that those who should be ignorant of this fact would place themselves in the very direction of death, while imagining that by so doing they were escaping the danger. The natives, by keeping always in front, are enabled to destroy it without much risk." One taken by Mr. B. measured in the thickest part seven inches in circumference, and three feet seven inches in length; and, by its disproportionate thickness, may easily be distinguished from all the others of this country. "I have," says he, "seen one about four feet and a half long, which, probably, is the greatest size it ever attains. The general colour is a dusky brown, but varied with black and cream-coloured transverse stripes, in shapes of which it is not easy to convey an idea by mere description."

CURIOUS INSTINCT OF THE COMMON HOG (*Sus Scrofa*, Linn.).

It is customary with farmers who reside in the thinly settled tracts of the United States, to suffer their hogs to run at large. These animals feed upon acorns, which are very abundant in our extensive forests, and in this situation they often become wild and ferocious. A gentleman of my acquaintance, while travelling some years ago through the wilds of Vermont, perceived at a little distance before him a herd of swine, and his attention was arrested by the agitation they exhibited. He quickly perceived a number of young pigs in the centre of the herd, and that the hogs were arranged about them in a conical figure, having their heads all turned outwards. At the apex of this singular cone, a huge boar had placed himself, who, from his size, seemed to be the master of the herd. The traveller now observed that a famished wolf was attempting by various manœuvres to seize one of the pigs in the middle; but wherever he made an attack, the huge boar at the apex of the cone presented himself—the hogs dexterously arranging themselves on each side of him, so

as

as to preserve the position of defence just mentioned. The attention of the traveller was for a moment withdrawn, and, upon turning to view the combatants, he was surprised to find the herd of swine dispersed, and the wolf no longer to be seen. On riding up to the spot, the wolf was discovered dead on the ground, a rent being made in his side, more than a foot in length;—the boar having, no doubt, seized a favourable opportunity, and with a sudden plunge dispatched his adversary with his formidable tusks.

It is a little remarkable that the ancient Romans, among the various methods they devised for drawing up their armies in battle, had one exactly resembling the position assumed by the swine above mentioned. The mode of attack they called the *Cuneus*, or *Caput porcinum*.—*Silliman's Journal, Jan.* 1822.

LUSUS NATURÆ.

At a place called Keene, in the United States of America, in April last a remarkable calf was taken from a heifer owned by a Mr. D. Clark, having no less than *eight* legs, *two* bodies, one head, *three* tails, and a large trunk (as the account states) measuring three feet! Its skin is undergoing a partial dressing, when it is to be stuffed in its true and perfect shape, and exhibited for the gratification of the public.

Mr. Ambidge, of Stowell Park, near Northleach, has now amongst his fine flock of sheep, a lamb with three shoulders and five legs; it is, though of so extraordinary a form, now doing well, and likely to live.

NATURAL CURIOSITY.

At Fawley, near Stanswood Mill, in the New Forest, is a floating island, upwards of two acres in extent, covered with trees of alder and willow, situate in a large piece of water called Pondhead, which was detached from the land during a high wind which occurred on Shrove Tuesday in 1781; it has continued floating since that time, and being shifted by the wind in its various directions, it is sometimes close to the road, and at other times a distance from it.

ORNITHOLOGY.

Shrewsbury, May 12, 1822.

Among the many interesting subjects which still remain open for research, is that of the natural history of Birds. Amongst others the Siskin (the *Fringilla Spinus*, of Linnæus; *Le Taun*, of Buffon) is peculiarly worthy of our remark. This little bird was seen in the early part of April, hopping from twig to twig in the gardens of Dr. Butler and Mr. Johnson, of the Flash. Its length is nearly five inches; bill white; eyes black; top of the

head and throat black; over each eye a pale yellow streak; back of the neck and the back yellowish olive, faintly marked with dusky streaks down the midddle of each feather; rump yellow; under parts of a pale green; palest on the breast; thighs gray, marked with dusky streaks; greater wing coverts of a pale yellowish green, and tipped with black; quills dusky, faintly edged with yellow; the outer web of each at the base is of a fine pale yellow, forming, when the wing is closed, an irregular bar of that colour across it; the tail is forked, the middle feathers black, with faint edges, the outer ones of a bright yellow, with black tip; the legs pale brown; claws nearly white.

Buffon observes that flights of these birds are only seen once in five or six years. They are not known to breed in this island, nor is it said from whence they come over to us. They are called Aberdeomes in the neighbourhood of London.

Yarmouth, May 26.

The nidification in this country of the *Parus Ptarmicus* has long been a subject of doubt with ornithologists: this season has brought the hidden subject to light through the exertions and perseverance of that indefatigable naturalist and bird-preserver, Mr. W. D. Ayers: the nest was placed about eighteen inches from the surface of the water, and composed principally of decayed summits of *Arundo Phragmitis*, and other aquatic plants; it was supported by a number of plants, curiously entwined, forming a very permanent support.

NEW COMET.

A new comet was seen by M. Pons (of Marlia) on the 14th of May, in the constellation *Auriga*. Its course was northerly, and when last seen was near ϑ *Aurigæ*. It was equal to a star of the 4th magnitude: and it is somewhat singular that it has not yet been seen in this country. In fact, we cannot learn that it has yet been seen by any one except the original discoverer. It may be proper to add that this comet is not the one which was expected to appear this year, as it is going in a contrary direction.

The *Journal des Debats* of the 20th May gives the following: —" M. Gambort jun., Adjunct Astronomer at Marseilles, discovered on the 12th inst. a new comet in the vicinity of the Second Star of *Taurus*. This comet was perceived yesterday at the Royal Observatory in this city (Paris), and the result of the observations which were made, showed that at forty minutes past ten o'clock it had about 87½ degrees of right ascension, and 36 degrees of boreal declension. The comet is at present invisible to the naked eye; its nucleus is small and brilliant; its atmosphere of little extent, and its tail scarcely perceptible."

NEW

NEW COMPASS.

Mr. William Clark, a messenger in Chatham Dock-yard, has invented a mariner's compass on an entirely new principle. The needle consists of four arms or poles, placed at right angles, and uniting in one common centre. The two northern poles are secured to the N.W. and N.E. and the two southern poles to the S.E. and S.W. points of the card, which places the four cardinal points right between the angles of the needle, and allows the card to point north and south as heretofore, the cards now in use answering the purpose. This compass has been tried under different circumstances, and, as far as can be ascertained by the experiments already made, is allowed to possess the principles of polarity and stability beyond that of any compass now in use.

NEWLY INVENTED MUSKETS.

[From the New York Evening Post of April 10.]

A curious invention in fire-arms has lately been accomplished by an ingenious mechanic of this place, by the name of Isaiah Jennings; and in point of importance, both for public and private use, it is probably not equalled by any invention of the present age. It is a single barrel and lock, stocked in the usual manner, and is perfectly simple, safe, and convenient. The number of charges may be extended to fifteen or even twenty—each charge being under as complete controul as a single charge in an ordinary gun; and may be fired in the space of two seconds to a charge, or at longer intervals, at the option of the possessor, with the same accuracy and force as any other gun. The principle can be applied to any musket, rifle, fowling piece, or pistol, and can be made to fire from two to twelve times, without adding any thing to the incumbrance of the piece, except five or six ounces to its weight. Thus the soldier is put in possession of a gun, out of which he can throw twelve or fifteen charges at his enemy, at the commencement of an engagement, as fast as he can cock and pull trigger, and be left in possession of a simple gun, to load and fire single charges like any other gun, with the advantage of its priming itself. The cavalry may be furnished with holster pistols containing five or six charges, which can be used on horseback, with the same convenience as ordinary pistols. The navy can be furnished with muskets for marines in close engagements, and boarding pistols, unequalled by any thing in naval warfare. In defending a breach, the power of ten men is concentrated in one; and in arming our small garrisons on the Indian frontiers, their power might be increased fourfold at an inconsiderable expense. And as a defence against the pirates that now throng our neighbouring waters, two or three of these guns, on board a

 merchant

merchant vessel, in the hands of skilful marksmen, would be able to cut off a whole boat's crew before they could succeed in boarding a vessel.

As a sporting or hunting gun, its advantages are not less important. It enables the sportsman to meet a flock with twice the advantage of a double barrel gun, without any of its incumbrances, and it enables the hunter to meet his game in any emergency. This gun has been shown to many officers of our army and navy, and has been highly approved of, and indeed no one who has seen a fair trial of its powers has ever been able to find an objection to it.

PRESERVATION FROM LIGHTNING.

Sir H. Davy, in his fourth lecture at the Royal Institution, recommends the following means of escaping the electric fluid during a thunder-storm. He observed that in countries where thunder-storms are frequent and violent, a walking-cane might be fitted with a steel or iron rod to draw out at each end, one of which might be stuck into the ground, and the other end elevated eight or nine feet above the surface. The person who apprehends danger should fix the cane and lie down a few yards from it. By this simple apparatus, the lightning descends down the wire into the earth, and secures him from injury.

EGYPTIAN ANTIQUITIES.

A considerable addition has lately been made to the Antiquities deposited in the British Museum from Thebes, Memphis, and other parts of Egypt. They are dispersed for the present in different parts of the Museum till provision can be made for their proper arrangement. There are in a room below the building, a Typhonic statue imperfect, in as much as the right elbow and both the feet are wanting, holding the *lotus* stem in full blossom: remains of an elliptical globe crown the head.—A piece of rough Egyptian or Ethiopian marble, apparently part of a frieze, covered over on one surface with hieroglyphics in the running-hand of that character.—A portion of a frieze of a temple (red granite), its interior or projecting underside with figures in high relief, among which a vessel brim full of water, dropping its contents, being super-charged with abundance; exterior surface covered with linear symbols.—Remains of a colossal female statue, in white lime-stone or marble, including the bust to middle of waist. A leaf of *lotus* ornaments her forehead; beautiful workmanship, and finely expressive of Ethiopian beauty.—A figure in Egyptian lime-stone, or white coarse marble, representing a body swathed for rest, or for a funeral.—A lower portion, containing the legs, of a red granite statue.—A piece of yellow marble,

marble, apparently from age, which seems to have constituted one of the sides of a *votive altar*, with a portion of three diminutive naked figures, in basso relievo, carved in a square on its surface, imperfect from being broken. Some Coptic characters inscribed.—Remains of a male colossal statue from the head to the bottom of the thorax. The root of lotus ornaments the forehead.—A remnant of pedestal of a statue, with remains of left foot, finely executed in red marble, or a very fine silicious stone: border inscribed with hieroglyphics.—A head of a finely carved female statue of large proportion.—The trunk of a female figure, delicately proportioned, apparently the produce of a Greek chisel.

In a small court behind the chief building, and by the side of the Athenian Gallery, there are fifteen remnants of female Typhonic statues, all charged with stems of the blowing lotus in the one hand, and having in the other hand the *Tau* or *Nilometer*, of nearly as many different proportions, and quite dissimilar as to remaining portions of the figure.—Two Egyptian or Ethiopic Graces (*Charities*), with either of them alternately having thrown their hands and arms behind the shoulders of its fellow (in red granite).—A red granite head of an Egyptian youth.—Remnant of a very large colossal head, perhaps a portion of a statue: the face is about four feet long by three broad, and its members proportionate, and delicately beautiful.—Another colossal head of same material.—Four remnants of clustered columns, each formed of eight smaller ditto, like the pipes of an organ, ensculptured with hieroglyphics. And various other remnants too numerous to describe.

In the Entrance Hall there are two statues of male Typhons, sitting on thrones, with *Tau* in left hand, which their knees support; heads crowned with elliptical globes (black granite).—An immense colossal head of nearly the same proportion with that already described, of singular beauty (red granite).—A female statue of ordinary proportion, with the head of a Jupiter Ammon upon her knees; her throne has many hieroglyphics (lime-stone apparently is the material of which it is made.)—An Ethiopian head of large proportion, beautiful countenance (white marble). An Egyptian sorceress, in a crouching attitude, sitting upon her heels; her mantle covered with symbols, or hieroglyphical figures (basalt).—A considerable circular vessel, about three inches deep, border inscribed with symbolical characters.—A considerable sized Egyptian (red granite) coffin, with its usual lid, having a carved resemblance of the person whom it contained, covered with hieroglyphics, very imperfect from the effect of weather.

EARTH-

EARTHQUAKE AT ZANTE.

At the time when the desolating earthquake that occurred in Zante in the end of 1820 took place, a remarkable circumstance was observed just preceding the shock. Three or four minutes before, there was seen at the distance of two miles from the point or promontory of Geraca, which is to the S.E. of the island, a kind of meteor burning and almost swimming on the sea, and which continued luminous five or six minutes. At the distance from which it was seen, it seemed to be five or six feet in diameter. Could this be hydrogen gas emanating from some volcanic submarine cavern, and which issuing out of the water in an aëriform column, sought to come in contact with the electricity of the atmosphere? This gas taking fire, continued to burn till the inflammable matter was consumed.—*Edin. Phil. Journ.* vi. 22.

CANAL STEAM NAVIGATION.

With a view to the introduction of steam vessels on canals, a very interesting experiment was made in the Union Canal at Edinburgh, on June 22, at two o'clock, with a large boat 28 feet long, constructed with an *internal* movement upon the principle of a model invented by Mr. Wight, and exhibited to a General Meeting of the Highland Society of Scotland in the month of January last. A Committee appointed for the purpose by the Directors of the Highland Society attended to witness the experiment, and the Chairman and most of the members of the Union Canal Company were also present. The boat had twenty-six persons on board; and although drawing fifteen inches of water, she was propelled by only four men at the rate of between four and five miles an hour, while the agitation of the water, being confined entirely to the centre of the canal, was observed to subside long before it reached the banks, and consequently obviating its hitherto destructive tendency in washing them into the canal.—*Star.*

IRON STEAM BOATS.

An iron steam boat has recently made a voyage from London to Rouen in 55 hours, and then proceeded to Paris. This we believe is the first attempt made to traverse the ocean in a vessel composed of any material but wood.

The enterprise has proved decidedly successful. Thus we have a direct communication open between the two great capitals of Europe, and which is performed in a shorter time than is taken by the stoutest merchantman to sail to Rouen only.—Not the

slightest

slightest accident occurred during the voyage, nor could the people on board discover any difference in the movement of the vessel from that of the strongest vessel that navigates the seas.

The *Tyne Mercury* mentions a new iron boat having been launched at Newcastle, thirty-one feet in length, which draws only two inches of water.

LIST OF PATENTS FOR NEW INVENTIONS.

To Henry Septimus Hyde Wollaston, of Clapton, Middlesex, for a bolt or fastening particularly applicable as a night bolt.—Dated 4th June 1822.—2 months allowed to enrol specification.

To William Huxham, of Exeter, iron-founder, for improvements in the construction of roofs.—4th June.—6 months.

To Henry Colebank, of Broughton Furness in the parish of Kirkby Ireleth, Lancashire, tallow-chandler, for a new and useful engine lately constructed by him, and now in his possession, for the purpose of cutting, twisting, and spreading of wick used in the making of candles, by which a great saving of manual labour is accomplished.—4th June.—2 months.

To John Barton, Deputy Comptroller of the Mint, for a certain process for the application of prismatic colours to the surface of steel and other metals, and using the same in the manufacture of various ornaments, which he conceives will be of great public utility.—4th June—2 months.

To James Frost, of Finchley, Middlesex, builder, for a new cement or artificial stone, which invention he believes will be of general benefit and advantage.—11th June.—2 months.

To William Feltham, of Ludgate Hill, stove-maker and furnishing ironmonger, for certain improvements on shower baths.—13th June.—6 months.

To Denny Gardner, of Edmund-place, Aldersgate-street, Middlesex, manufacturer, for a stay particularly applicable to supporting the body under spinal weakness, and correcting deformity of shape.—13th June.—2 months.

To Joseph Wass, of Sea Wharf, Ashover, Derbyshire, millwright and lead-smelter, for an improvement which prevents the ill effects to vegetation and animal life that have hitherto been occasioned by noxious fumes and particles that arise from smelting or calcining lead ore and other pernicious minerals.—15th June.—6 months.

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand.

Days of Month. 1822.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Weather.
May 27	55	65	55	30·14	Fair
28	55	69	60	·25	Fair
29	60	72	60	·28	Fair
30	59	70	61	·35	Fair
31	61	74	62	·32	Fair
June 1	63	76	62	·23	Fair
2	62	75	60	·28	Fair
3	60	75	62	·29	Fair
4	62	80	61	·23	Fair
5	64	78	65	·24	Fair
6	66	76	64	·21	Fair
7	60	73	55	·21	Fair
8	54	73	63	·17	Fair
9	64	79	69	29·97	Fair
10	67	84	68	30·04	Thunder in the evening
11	66	76	62	·17	Fair
12	62	69	55	·24	Fair
13	56	69	60	·24	Fair
14	62	77	67	29·92	Fair
15	66	70	56	·73	Thunder-storm
16	56	67	55	·96	Fair
17	57	66	55	30·25	Fair
18	57	69	59	·21	Fair
19	60	75	60	29·97	Fair
20	59	64	55	30 08	Fair
21	55	65	55	·22	Fair
22	55	74	60	·14	Fair
23	59	74	63	·06	Fair
24	60	73	66	·10	Showers
25	64	77	66	·15	Fair
26	66	77	66	·04	Fair

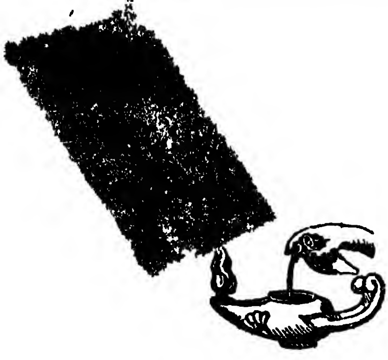
N.B. The Barometer's height is taken at one o'clock.

Erratum in our last Number.

Page 336, line 21 (of the Letter) article Caoutchouc, *for* with such power, &c. *read* with such pieces Mr. Moore says he has made air-balloons.

INDEX TO VOL. LIX.

END OF THE FIFTY-NINTH VOLUME.



R. and A. Taylor, Shoe Lane.

N°1.

Specimens taken from the outside of the Wood merely pieces of the root

Fig 1

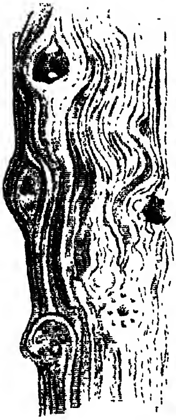


Fig. 2

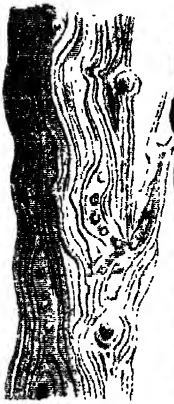


Fig 3.

Fig 4

Fig 5

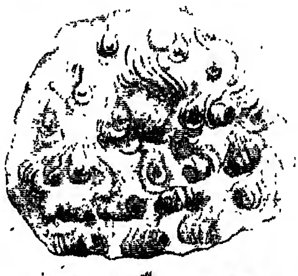


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Fig 6.

Fig. 10.